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AN EXPERIMENTAL INVESTIGATION OF THE DETERMINANTS OF APPARENT VISUAL MOVEMENT

By HARRY R. DESILVA, Harvard University

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It is a well-known fact that a perception of movement may be experienced between two stationary objects if the interval of their appearance is regulated within very brief limits of time.[1] In this study an endeavor has been made to discover the principal determinants or conditioners of this phenomenon of visual

[1]The best summary of work done upon the perception of apparent movement is contained in an article by H. Helson, Psychology of *Gestalt*, this JOURNAL, 36, 1925, 498-514. Other relevant information is to be found in his doctoral dissertation on *The Configurational Theory of Perception* in the Harvard University Library.

Other articles are: M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-278. K. Koffka, Perception, An Introduction to *Gestalt-theorie*, *Psychol. Bull.*, 19, 1922, 548-550; *Beiträge zur Psychologie der Gestalt* (containing articles also by F. Kenkel and A. Korte), 1919; with P. Cermak, Untersuchungen über Bewegungs und Verschmelzungsphänomene, *Psychol. Forsch.*, 1, 1921, 66-129. E. Lindemann, Experimentelle Untersuchungen über das Entstehen und Vergehen von Gestalten, *Psychol. Forsch.*, 2, 1922, 5-60. F. L. Dimmick, this JOURNAL, 31, 1920, 332-333; F. L. Dimmick and G. H. Scahill, *ibid.*, 36, 1925, 412-417. G. D. Higginson, The Visual Apprehension of Movement under Successive Retinal Excitations, *ibid.*, 37, 1926, 63-115 (excellent bibliography).

movement, by using a tachistoscope in which certain objective factors in the stimulus-situation could be varied, and by carefully controlling other conditions under which the experience of movement arises.

This investigation has attempted to examine only some of these variable factors which give rise to and govern the experience of movement; the possible determinants appear to be so numerous and so difficult to describe and control that we are apparently still a long way from any final understanding of the phenomenon. Among the many variables, attention has been devoted chiefly to a study of the factors of repetition, fatigue, background, position, form of stimulus, meaningfulness of stimulus, encompassment of stimulus, fixation point and predispositions of various sorts.

Apparatus. A modification of the Dodge[2] tachistoscope (depending upon the principle that a glass plate is transparent when the illuminated object lies behind it, while the same plate functions as a mirror when the illuminated object lies in front) was constructed for this investigation. The accompanying diagram (Fig. I) indicates the arrangement of the apparatus.

The body of the tachistoscope was made of wood and painted black within and without except for white passageways leading from the stimuli to the source of illumination. Three 75-watt, Type-C Mazda, frosted, nitrogen-filled lamps (B), placed opposite the ends of the three parallel tubes furnished the illumination which was reflected indirectly upon the exposure fields. Aubert diaphragms (AD) were set in the tubes opposite the lights in order to equalize the illumination. Ground glass plates (W) placed in the passageways helped to render the illumination more even. The projections seen on the sides of the passageways in the diagram indicate screens containing square openings which served to confine the illumination to its proper path. Cards (S1, S2, S3) were inserted in slides through slits cut in the solid board top covering the entire tachistoscope. The optical distance of every stimulus from the eyes was 15½ in. and the size of the stimuli was such that with central fixation the physical stimulus pattern fell upon the retina within the area of central (foveal) vision.

Attached to the tachistoscope at the open end was a frame with brass grooves in which a falling shutter (steel) operated. The shutter (FS) fell from a constant height by its own weight when released by a catch. Its fall was checked by a padded platform on three spiral springs. The shutter was raised by means of a cord passed over a ball-bearing pulley.

The duration of the exposure was regulated by means of adjustable cardboard windows on the shutter. Exposure intervals were computed, regulated and tested from time to time. Contacts made at the beginning and during the fall actuated a Johns Hopkins (Dunlap) chronoscope,[3] thus giving the time of fall, which was found to be remarkably constant. The whole interval of fall varied no more than 10 to 15 σ, making the *Zwischenzeit* vary little more than 2 σ.

The *Zwischenzeit* was illuminated and was made standard at 60 σ for all experiments. The exposure time of both the first and second stimuli was 65 σ.

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[2] R. Dodge, *Psychol. Bull.*, 4, 1907, 10-13.

[3] K. Dunlap, *J. Exper. Psychol.*, 2, 1917, 49-52.

The tachistoscope was located in a window between two dark rooms. *O* remained in the darkness during the whole period.

Stimuli. Lines drawn on oblong cards were used for stimuli. The length of a radial line was 1 cm., of a diametral line 2 cm.; the width of lines was 1 mm. The size of the total visual field was 7 x 7 cm. Throughout the experiment the fixation point was always made to coincide with the junction of the two lines.

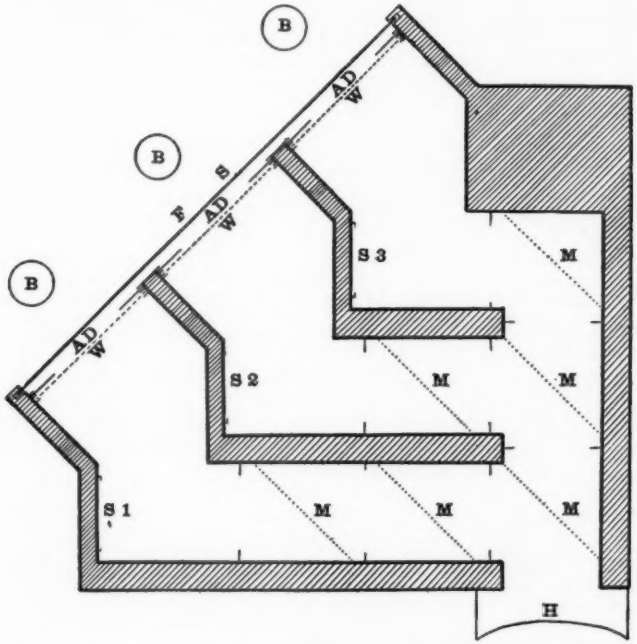

FIG. I. TACHISTOSCOPE. The diagram gives an overhead cross-section view of the tachistoscope. B: electric lamp bulbs. FS: falling shutter. AD: Aubert diaphragms. W: glazed windows. S_1, S_2, S_3: stimuli. M: transparent glass plates. H: hood for *O*. The short lines projecting from the side walls represent large pieces of cardboard with square openings in the center. These screens serve to limit the visual field of the *O*, and to confine the illumination to its proper path.

Observers. The regular *Os*, three in number, were untrained in systematic introspection. They were A. R. Stone (ST), T. Frank (FR), first year graduate students in psychology, and J. P. Bethel (BE), graduate student in English. The *Os* were kept ignorant of the nature of the experiment and requested not to talk among themselves or to other people concerning it. They reported regularly three hours a week during the collegiate year from the last of September to the middle of March.

I. Nature and Development of the Perception of Movement

The data of the experiment reported upon in this section were gathered from fourteen *O*s during their first hour of observation of the phenomenon of visual apparent movement. The individual differences in the adaptation of the *O*s proved to be interesting, and the protocols derived from this experiment contain much relevant information concerning the nature and development of the perception of movement and its dependency upon certain determinants, of both an objective and subjective character.

Observers. Besides the regular *O*s, eleven casual *O*s contributed one hour apiece for this experiment. They were D. K. Adams, C. Goldthwait, H. Gurnee, H. Hoagland, E. D. Hutchinson, R. A. McFarland, J. M. McGinnis, C. C. Pratt, M. Upton, W. Vaughan, K. E. Zener. With the exception of Dr. Pratt and Mr. Goldthwait all *O*s were relatively untrained in experimental work.

Procedure. At the outset the *O*s were charged with the following simple instructions: "Fixate the dot before you. You are going to be shown something. Please observe it carefully and report what happens. Under no circumstances allow your gaze to wander from the fixation point. You will find you will be able to see what happens without taking your eyes off the fixation point, so guard against it."

Every effort was made to avoid the influence of suggestion, but occasionally throughout the investigation *E* attempted to follow up and elicit more complete information where data seemed scanty, and where *O* appeared to be unduly parsimonious. For instance, when the phrase 'flops down' was used two or three times by an *O*, it was incorporated into a casual question concerning the 'flopping down.'

The work was arranged as far as possible so as not to fatigue *O* and he was cautioned to notify *E* of the first moment of weariness. Effort was made to avoid 'pressing' *O* too closely for a precise or exact description. In general, *O* was left to report the experience as it occurred to him. Reports were given orally by the *O*s and written down by *E*. After the experiment was over the casual *O*s were questioned informally about the experiences during the whole hour and encouraged to relate everything that occurred to them regardless of whether they considered such information relevant or irrelevant.

Stimuli. The stimuli consisted of a radial arrangement (No. 1 in Fig. II) making an angle of 70° to the right, a diametral arrangement (No. 2, Fig. II) of the same pattern, and a pictorial arrangement of an encounter of an old woman with a dog.

Protocols. Because of lack of space the introspections of the *O*s are omitted and only a summary of the results from introspections is given.

Summary. (1) Usually at first the *O*s are confused and respond as follows: "The second line flashed in," "dropped in," "appeared from above," "it's too fast, I don't know," "horizontal appears, vertical appears," "diagonal thrown in," "flopped in," "fades in," "horizontal vibrates and then seems to fade." The apparent movement is obviously artificial under tachistoscopic conditions; moreover, the distractions of the walls of the

tachistoscope and the slight flashing of light tend to render the report of *O* artifictitious—at least in the beginning.

(2) On continued observation the *O*s often eventually fall of their own accord, into report in transitive terms connotative of continuity, using the word 'movement' seemingly because it is the most natural and simple and obvious, and perhaps also because there is an experimental setting and sanction for the relationship between lines in space and time. There is a certain change in the attitude of every *O* above and beyond what would

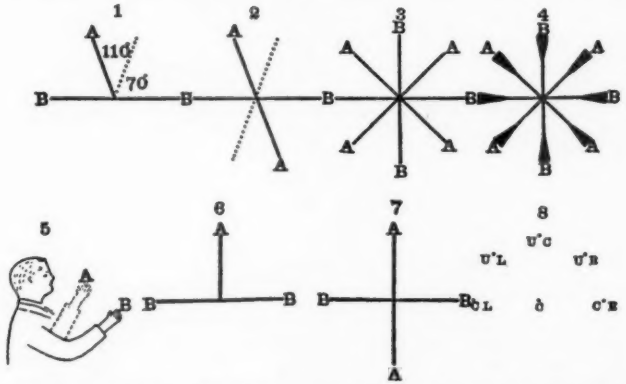

FIG. II. STIMULI. The eight diagrams show the form and geometrical relationships of the stimuli, and reference in the text is repeatedly made to this figure. The first seven diagrams represent successive exposures of a pair of stimuli: the initial position is marked 'A' and the succeeding position is marked 'B'. Diagrams 1 and 6 are for 'radial' movement; diagrams 2, 3, 4, and 7 are for 'diametral' movement. Diagram 4 is the 'windmill' figure. Diagram 8 shows the positions and corresponding symbols of the 6 fixation points used (Section VI) when fixation point was varied.

be expected from mere habituation. With most *O*s we might describe this change as a loss of 'criticalness', of 'uncertainty', of 'indeterminateness', and a general narrowing of the field of attention.

(3) There is a gradual development of the 'formedness' or 'togetherness' of the apparent movement with practice and training. The *O*s become more receptive of the movement and less attentive to other artificial disturbances. During a single hour even there is an obvious development in the ease of perceiving the movement of the lines, rather than mere perception of succession. As an *O* grows habituated to the artificial laboratory conditions and regards the successive appearance of

the bare lines as a meaningful, eventful situation the movement becomes thereby more nearly univocal and more distinct. In those cases where the meaningful situation was used, the configurational effect of the addition of meaning was patent. In every case where meaningful familiar objects were shown, the *O*s reported 'movement' either explicitly or by synonym, without the least hesitation, but in the case of bare lines the movement was not so clear. Thus it would seem that by clothing bare figures and lines with purposefulness or meaning we thereby render them better qualified to evoke movement experiences.

(4) At first, certain *O*s (proponents of *Gestalttheorie* might call them analytical *O*s) did not perceive the movement and continued for some while to speak of lines 'appearing' and 'flashing' without reference to phenomenological continuity. When, however, such *O*s were shown meaningful stimuli under the same conditions, they always said "it did so and so" or something to the same effect, implying that it moved. After seeing meaningful objects move under the same conditions and classifying the process as movement, the *O*s usually did not hesitate thereafter in applying the term 'movement' to the successive appearance of lines.

(5) The ends of the lines carry the movement. That part of the line nearest the axis of movement is comparatively unimpressive and unnoticed. Moreover, in the case of diametral movement the upper phase of the movement seems to be the one to which the naïve *O* naturally directs his attention. The lower phase of the movement is sometimes noticed, but the movement of the upper end of the line is the most prominent and impressive.

(6) The *O*s being questioned from time to time as to the positiveness and certainty of their judgments in the case of the equivocal situation, report that their conclusions are reliable and valid, and not 'forced' or 'arbitrary'. A quick judgment was usually made. When a judgment was slow the *O*s usually admitted uncertainty. They were repeatedly encouraged to report 'don't know' rather than give a reflective judgment.

(7) On several occasions *O*s reported that they were sure of the motion but not of the order of appearance of the lines or of any lines at all. This experience which came involuntarily and irregularly we take to correspond to the 'pure' *phi*-phenomenon[4] in which there is no synthesis of content, no founding of elements and no sensational attributes. With untrained *O*s it could not be obtained by attitude or 'trying'. It was something that appeared of its own accord at very irregular intervals.

[4] *Cf.* M. Wertheimer, *op. cit.*, 215, 227. Also, V. Benussi, Versuche zur Analyse taktil erwekte Scheinbewegung, *Arch. f. d. ges. Psychol.*, 36, 1917, 65 ff. Benussi's *O*s were unable to experience 'pure' *phi* movement.

(8) Some of the *O*s occasionally reported movement in the reverse direction from which it actually occurred. In other words they saw first the line which actually appeared second.[5] It was noted that the movement was reversed mostly in a counterclockwise direction. The confusion of the sequence of lines, the reversal of the direction of the movement, and the corresponding loss of compulsoriness of the direction of the movement, occurred only in the early stages of training and under conditions of fatigue.

(9) In experiments of this kind involving uncontrolled attitudes it is always important to guard against artifacts. An attempt was made to avoid 'forcing' an analytical, process attitude upon an *O*. Only two *O*s ventured free rising analytical reports as to the nature[6] of the movement experience and we did not venture to ask anything further concerning the exact nature of the perception except in the case of these two men.

ZE characterized the experience of movement, that is to say, what was taking place in the arc of movement, as a "change in brightness, change of intensity. Something seems to happen there. Not black and not white. Like heat rising on a hot day from the p avement. But we can usually see heat waves better. This thing is much faster and more elusive." Upon being asked if he had anything more to say about the nature of the movement he remarked that "it was very difficult to see and much more difficult to describe."

CP adopted an analytical attitude of his own accord and gives us a much completer report: "I can only describe the movement by analogy. The pure movement has an appearance more of an imaginal type. Something more or less imaginary. Something more like the trail of a match in the dark. I can't follow the movement of the line through space. I can get something in terms of imagery, a fuzzy sort of thing that comes in the arc of the movement. It is different from the blackness of the line. It isn't as black and as 'clear-cut'." Later on: "a kind of blurring after-effect." CP also remarked upon the extraordinary difficulty of ever being able properly to get at it descriptively. He seemed to think of the actual stimulus as being almost beyond the ability of the sense-organs and the perceptive process to grasp.

(10) At first nearly all the *O*s seemed to set themselves to experience the movement analytically. With the passage of

[5]This phenomenon corresponds to Korte's reverse movement (*Zsch. f. Psychol.*, 72, 1915, 204). By repeating Korte's conditions, that is to say, making the second member wider and more 'intense' than the first, and by turning the attention to the second member, the direction of the movement was found to be reversed. Moreover, the movement was quite positive. Dimmick (*op. cit.*, 31, 1920, 331) reported that his *O*s were unable to get positive movement perceptions under such conditions.

[6]Dimmick (*op. cit.*, 331-332) states: "The temporal attribute is beaten up with the grey quality into an integration of the first order. All *O*s describe the experience as a 'flashing', 'flickering', 'unsteady', 'shimmery', 'flame-like', 'liquid', 'live' grey." Whether the description of the Cornell study is ultimate or representative of the immediate bare experience is to be questioned.

time and the recurrence of the stimulus-situation the general perceptual pattern or configuration seems to change, to become more unified and to yield a more nearly univocal and synthetic experience. The change of set seems to be due as much to non-stimulus determinants as to stimulus determinants. To *E* this indicates that the general perceptual pattern or set of *O* is of even greater significance and relevance in the study of movement phenomena than it is in the study of more static experiences. The 'analytical' and 'totalizing' attitudes seem to be quite variable. *O*'s attitude toward movement phenomenon varies in degree as well as in time. Consequently, greater efforts than usual must be made to control attitude in investigating the experience of movement lest we merely build up a system of artifacts which will be of no use to psychology.

II. Influence of Repetition, Fatigue, Background, Form of Stimulus and Meaningfulness of Stimulus

This experiment is an outgrowth of the work reported upon in the preceding section and consists in a more careful and systematic examination of the factors of repetition, fatigue, background, form of stimulus and meaningfulness of stimulus, in an attempt to discover and describe their influence as determinants of the phenomenon of movement. Moreover, the experiment is intended to throw light on the development and decay of the configuration or pattern of movement, in so far as it is manifested in a change of clearness during repetition.

Instructions. "You will be shown a certain movement a number of times. At the end of every 5 exposures when you hear the signal 'now' make a judgment on the basis of the clearness of the movement experience according to the following scale: Report: 1, if the movement has maximal clearness; 2, if the movement is clear but not quite maximally clear; 3, if the movement is moderately clear; 4, if the movement tends to be vague and uncertain yet fairly definite; 5, if the movement is positively unclear, uncertain, and you are not even sure which way it goes."

Later during the preliminary trials when *O* was presented with a different arrangement, additional instructions were given, namely: "Let your judgment be a comparative one, that is to say in each series let 1 stand for the most clear movement, 5 for the most unclear movement. 1 and 5 of course need not correspond to each other exactly in any two arrangements, but if you note any marked differences in the maximal clearness between different series please report them. Should you judge 1 at the beginning of a series and later on experience what seems to you to be a clearer perception of the movement, report the fact so that we may correct the scale. However, do not confuse such a case with a case in which the movement is a 1, maximally clear, becomes a less clear 2, and once again becomes a 1 maximally clear."

The *O*s were encouraged to ask questions concerning the instructions. In no case did any *O* experience any noticeable difficulty in carrying out the instructions.

Procedure. The variations of stimuli and background as they pertain respectively to the data shown in Figs. III and IV are as follows:

1 a. Movement of a black radius (No. 1 in Fig. II) and a black diameter (No. 2, Fig. II) on white backgrounds. The arrangements were given in both the 70° acute angle position and in the 110° obtuse angle position (as indicated by oblique black and dotted lines in Nos. 1 and 2, Fig. II). The results represented in the curves of 1a are for ST.

1 b and 1 c. Same as above for BE and FR respectively.

2. Movement of two 'crossed' diameters (No. 3, Fig. II) on a white background.

3. Movement of a 'windmill' (No. 4, Fig. II) on a white background.

4. Movement of a man saluting on a white background (No. 5, Fig. II).

5. Movement of a white diameter on a black background. *Cf.* 1 a above for angle arrangement.

6. Movement of a black diameter on a grey background. Same.

7. Movement of a 'black crochet thread' diameter[7] on a white background. Same.

8. Movement of a 'white crochet thread' on a black background. Same.

Each angle arrangement was repeated in every case until *O* reported 5, or very unclear movement.

The results as graphically represented in 1 a to 8, Figs. III and IV, show the rise and decay of the perception of movement as based on the 5-point judgment scale. The dotted line represents 3 the average or mid-point on the judgment scale. Deviations of the curves above and below the line correspond to changes in relative clearness of the perception of movement.

Summary. (1) There is an increase in the clearness of visual perception of movement up to the 15th or 20th exposure, although the maximum may be reached as early as 10 or as late as 50. This increase in clearness, of course, is subjective and seems to consist of an increase in the perceptibility of the perseveration of the objective stimulus pattern. The duration of the immediate present of the perception of movement seems to increase at first and then with continued repetition to decrease.[8]

(2) The period of greatest clearness of the perception of movement lasts sometimes for only 2 or 3 exposures, sometimes

[7]The crochet thread diameters were constructed by passing crochet thread (the same width as the ink lines) through two small holes punched in the card, and fastening the loose ends on the back.

[8]It is interesting to compare this conclusion with the work of V. Benussi, Gesetze der inadäquaten Gestaltauffassung, *Arch. f. d. ges. Psychol.*, 32, 1914, 407-408, on the Müller-Lyer illusion, to the effect that voluntary exercise of a synthetic attitude is accompanied by fatigue of an analytic attitude and vice versa. Also that persistence in an attitude tends either to improve or destroy the illusion, according as one or other attitude becomes more and more dominant.

In the beginning of a series of movements under the experimental conditions described above a synthetic attitude seemed to predominate. The movement configuration improved until the onset of fatigue, whereupon the synthetic attitude seemed to break down with a resultant disintegration of the perceptual pattern of movement.

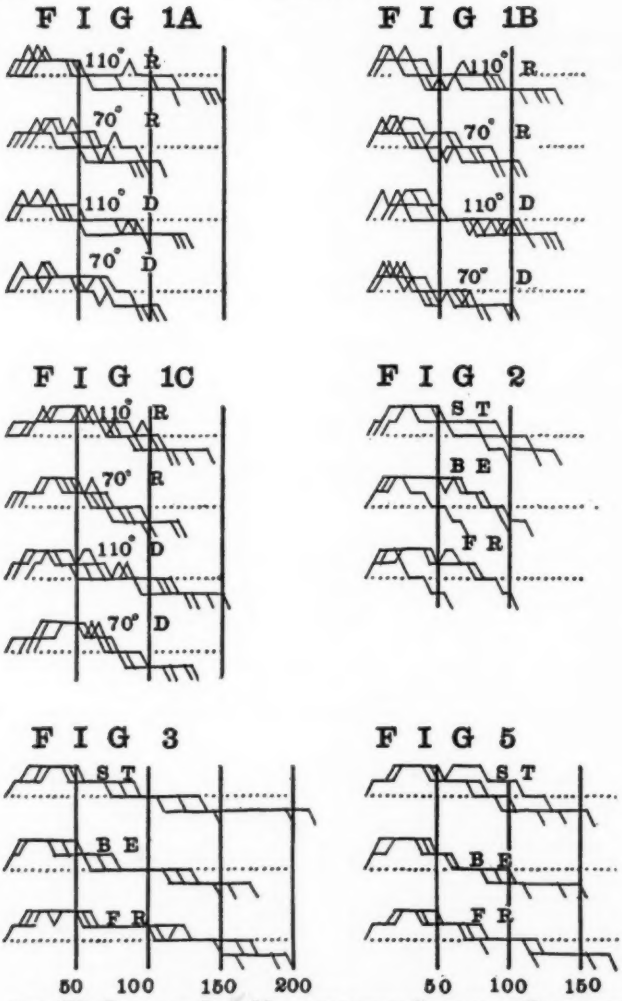

FIG. III. DECAY OF SEEN MOVEMENT WITH SUCCESSIVE EXPOSURES. See "Procedure" in text and Fig. II for illustrations of stimuli. The exposures were continuous and judgments were made every 5 exposures. Abscissae indicate exposures. Ordinates indicate judgments of the perception on the basis of 5 degrees of clearness. The highest points on the curves represent the judgment of 'very clear'; next level 'clear'; next level (corresponding to the dotted line) 'moderately clear'; next level 'vague'; lowest level 'unclear.'

for 60 or 70 exposures. Even when *O* strives to maintain a constant attitude, there is a very obvious irregular variation in the clearness of perceiving the movement. On many occasions the clearness of the experience of movement increased after it had once started on a decline.

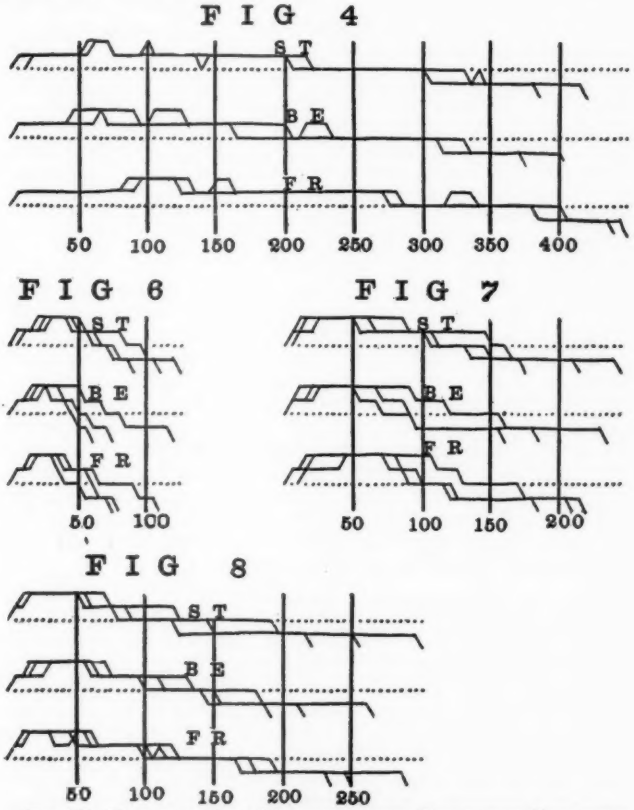

FIG. IV. DECAY OF SEEN MOVEMENT WITH SUCCESSIVE EXPOSURES. See legend to preceding Fig. III.

(3) After movement has become thoroughly unclear one may, by persistence of attention and voluntary effort, cause a revival of perceptibility which may last for 50 or 60 exposures. At about this point the eye-strain and fatigue become so great that the experiment has to be halted.

(4) The movement of a radius seems to persist as long as the movement of a diameter, but the former is never as definite as the latter. Radial movement, because of its 'bareness', simplicity and univocality, permits itself to be analyzed and experienced as 'succession' with greater ease than does diametral movement. It is much more difficult to maintain a constant passive attitude in observing the movement of a radius than in observing the movement of a diameter. Moreover, the diametral movement seems to be more impressive. There is an indication that diametral movement is in general more meaningful.

(5) The movement of two diameters fatigues more slowly than the movement of one diameter. This observation coincides with the results reported below in Section VII,[9] in which the movement of two diameters in the equivocal situation (where an object may move in either of two directions) invariably was found to be the more stable movement arrangement.

(6) The movement of a meaningful animate object is less fatiguing and more durable and insistent than the movement of a less meaningful inanimate object. In the case of the movement of the man saluting, the 'reality' and 'animateness' of the situation seems to hold and centralize the attention of the *O* with the result that the movement persists for a long time. The movement of the man's arm in saluting is a movement of 'something' and compels the attention, thus relieving the *O* from putting forth as much effort, and perhaps also causing him to disregard his fatigue. The movement in the meaningful situation is more real, is psychologically 'filled in' so to speak, and accordingly loses much of the mechanical and artificial character of the movement of 'bare' lines.

(7) The movement of the 'windmill' figure (No. 4, Fig. II) is more durable than the movement of lines and less durable than the movement of a more meaningful object. This difference may be accounted for on the basis of the relative meaningfulness involved. The arrangement with the thickened lines is more meaningful (like a windmill or a cross) than the 'bare' lines, but less interesting and meaningful than the arrangement of the man saluting. Moreover, the lines in the 'windmill' figure which carry the movement, are filled in at the ends. Anything that one does to direct the attention to the ends of the lines by filling them in, or by surrounding the arrangement by a circle,[10] tends to increase the clearness and durability of the movement. The lines thickened at the ends are more impressive than the uniform lines, and when repeated, decay much less

[9] 494 ff.

[10] Cf. Section IV below, 486 ff.

readily. The windmill movement has more 'body' to it. It can be perceived with less strain because it is more impressive.

(8) A white line on a black background produces a clearer and more durable movement than that produced by a black line on a white background. The white on black is more impressive. The movement is more striking and more deliberate. At first the movement of a white line on black was reported as 'comparatively slow.' With repetition, the movement was reported as 'faster.' As the eyes grew more fatigued the movement became faster and less definite. The movement of white on black is the clearest of all movements. In the case of black on white or of black on grey, with fatigue the black seems to blend with the background, but the white never blends on black even when the movement grows less clear.

(9) The movement of a black line on a grey background is the least durable of all movements. It is never so distinct as the movement on the black background and seems to blend with the background very quickly. The blending or fading takes place more suddenly with a grey background. The grey background proved very easy upon the eyes, producing no noticeable fatigue in spite of the fact that the movement became unclear so quickly.

The influence of background on movement as reported in this section is corroborated in other data reported in the section immediately following.

(10) The use of a solid crochet thread diameter rather than an ink line has the general effect of rendering the movement experience more definite and durable. The movement is more impressive and resists fatigue longer than in the case of an ink line.[11] A solid white object on black evokes the clearest movement. It easily arrests the attention and has a 'resiliency,' as one *O* expressed it.

(11) In 1 a, 1 b and 1 c, Fig. III, movement in a counterclockwise direction is seen to last longer than movement in a clockwise direction, indicating the presence of a predisposition in the *O*s for counterclockwise movement.

III. Influence of Background

Although attention focuses consciousness upon one specific aspect of a total situation, the margin of consciousness and the background of the stimulus-situation exercise a considerable in-

[11] A. Gelb and R. Granit (Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle, *Zsch. f. Psychol.*, 93, 1923, 103-106) found that the limens for figures were greater than limens for grounds, and that the higher values for lines of the figure showed that they resist change more than ground. In the above case, a thread is more *aufdringlich* than a line, actuates more increments of experience, partakes more of the nature of figure, and accordingly, its resistance to change is greater than in the case of a line.

fluence. All perceptual experiences have these two distinguishable aspects: a focal impressive structure with very definite features, and a general background from which the first raises itself. The distinction between the figure which appears and the less structured level may be denoted by the words 'figure' and 'ground'.[12] In this section we are concerned with the effects of variations in background on the perception of movement.

A careful study of the protocols of the *Os* in all experiments indicates that most of the experiences of movement emerge from a total situation and are influenced by the vehicle of movement and the background against which it moves. It is the contention of *E* that the color, consistency and obtrusiveness of the object which moves, and the brightness and impressiveness of the background are interacting factors which must be controlled in an investigation of movement phenomena. Even the 'phi-ness' of *phi*-phenomena seems to vary with the objective conditions of the stimulus and background as well as with the subjective attitude of the *O*.

Stimuli. The following stimuli were used:
(1) Black diametral arrangement on white background (No. 2, Fig. II).
(2) White diametral arrangement on black background (same).
(3) Black diametral arrangement on grey background (same).
(4) Black crochet thread diametral arrangement on white background (same).
(5) White crochet thread diametral arrangement on black background (same).

Procedure. The first arrangement was exposed a few times. Immediately after the last exposure of a series (*i.e.* as quickly as the cards could be changed, an interval ranging between 3 to 7 sec.), the second arrangement was exposed, and the *O* requested to "note all the differences he could between the movement in the first series as compared with the movement in the second series." Several changes of arrangements were made and the *O* given every opportunity to check up and verify any difference he noticed.

During a preliminary series of comparisons, all three *Os* reported something or other concerning a difference in the speed, in the clearness, and in the completeness of the movement. One *O* (BE) reported a difference in 'haziness' in the arc of the movement. Inasmuch as the report of these four differentiae were consistent, they were incorporated into the following *Aufgabe*: "You will compare and report upon the difference, if any, in the speed, the clearness, the completeness of the movement (whether you can follow it easily from beginning to end), and in the 'haziness' of the arc of the movement."

None of the *Os* experienced any particular difficulty in making judgments on the basis of these criteria. The same procedure of comparison was applied to the stimuli listed above, as follows:

Part 1. (1) White line on black background
(2) Black line on white background

[12]The importance of this division within perception is pointed out by E. Rubin, *Visuell wahrgenommen Figuren*, 1921, vii, 244, who found a characteristic set of properties for figure and another for ground. Koffka, *Psychol. Bull.*, 19, 1923, 567-569, generalizes this distinction by calling the ground a 'level' upon which the figure appears.

Part 2. (1) Black line on white background
(2) Black line on grey background
Part 3. (1) Black line on grey background
(2) White line on black background
Part 4. (1) White crochet thread on black background
(2) Black crochet thread on white background
Part 5. (1) White crochet thread on black background
(2) White line on black background
Part 6. (1) Black crochet thread on white background
(2) Black line on white background

Parts 1, 2, 3, 4, 5 and 6 of Table I summarize the reports of the *Os*.

TABLE I. INFLUENCE OF BACKGROUND ON PERCEPTION OF MOVEMENT. Each part of the table shows the results of a comparison of two stimuli, referred to as '1' and '2,' with respect to four criteria of differences

BASIS OF JUDGMENT	*Observer* ST	BE	FR
PART 1.	1: WHITE LINE ON BLACK GROUND. 2: BLACK LINE ON WHITE GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 more distinct	1 clearer	1 clearer
Completeness of movement	1 whole motion through arc	1 follows through better	1 movement completer
Arc of movement	2 hazier	2 hazier	2 hazier
PART 2.	1: BLACK LINE ON WHITE GROUND. 2: BLACK LINE ON GREY GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 clearer	1 clearer	1 clearer
Completeness of movement	Unable to make judgment	Unable to make judgment	Unable to make judgment
Arc of movement	1 hazier	1 hazier	1 hazier
PART 3.	1: BLACK LINE ON GREY GROUND. 2: WHITE LINE ON BLACK GROUND.		
Speed	1 faster	1 faster	1 faster
Clearness	2 clearer	2 much clearer	2 clearer
Completeness of movement	2 follows all the way	2 completer	2 completer
Arc of movement	1 much hazier	1 hazier	1 much hazier
PART 4.	1: WHITE THREAD ON BLACK GROUND. 2: BLACK THREAD ON WHITE GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 greater contrast	1 stands out	1 clearer
Completeness of movement	1 completer	1 follows through better	1 more definite
Arc of movement	1 no haziness	2 hazier	2 hazier
PART 5.	1: WHITE THREAD ON BLACK GROUND. 2: WHITE LINE ON BLACK GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 clearer	1 clearer	1 clearer
Completeness of movement	1 easier to follow	1 easier to follow	1 easier to follow
Arc of movement	2 hazier	2 hazier	2 hazier
PART 6.	1: BLACK THREAD ON WHITE GROUND. 2: BLACK LINE ON WHITE GROUND.		
Speed	2 faster	2 faster	2 faster
Clearness	1 clearer	1 clearer	1 clearer
Completeness of movement	1 follows through easier	1 completer	1 easier to follow through
Arc of movement	2 hazier	2 hazier	2 hazier

Summary. (1) Movement of white on a black background is clearer and easier to follow than movement of black on a white background.[13] A white line against a black background hesitates at first and then moves deliberately across the field. It is much more difficult to follow the movement of black on a white ground than to follow the movement of white on a black ground. An inverse relationship exists between speed and clearness. The faster the movement, the less clear the phenomenon. On the other hand, if the movement is slower it is more distinct and easier to follow. The movement of white on black is quite impressive and compels the attention more than movement of black on white.

(2) The movement of a black line on a grey ground is not quite as clear and distinct and as easy to follow as the movement of a black line on a white ground. There is, however, comparatively little difference. The grey ground is optically more restful than the white ground, but the *O*s were unable to come to a consistent agreement as to whether the movement was more complete in the one case than in the other. The movement on a white ground tends to be vague, but the movement on a grey ground is more vague. On grey the movement is reported faster than on white, an occurrence which is to be explained by the fact that the black blends a little better with the grey than it does with the white, and also by the fact that because the movement cannot be followed through completely it is judged to be more rapid. The conclusion seems to be, that when a moving object stands out distinctly against a ground we tend to judge it as moving slower than when it blends more completely with the ground.

(3) 'Thingifying' or 'objectifying' a line (*e.g.* thread) diminishes the speed of the movement and at the same time renders the movement clearer and more distinct and easier to follow. Moreover, the haziness in the arc of the movement is reduced. The solidity of an object makes it more real, giving it an *aufdringlich*, 'thing' character which sets it off more sharply against the ground. The movement of an ink-line partakes more of the nature of a 'flash.' The movement of a crochet thread approximates to the actual movement of a real object. There is something very substantial and durable and indubitable

[13] An unpublished experiment of L. Hartman, reported by Koffka, *op. cit.*, 565 ff., is interesting in this connection. Hartman worked with a Schumann tachistoscope with rotating disc (containing windows) instead of a falling shutter as used in this investigation. He discovered that the time of fusion as measured by the critical speed (that speed which is just capable of bringing about complete fusion after the last bit of flicker has disappeared) of a black cross on a white ground was greater than the time of fusion for a white cross on a black ground. This is used by Koffka to point out the close relation between 'formedness' and 'vividness.'

about the movement of a solid line. The perceptual experience attending the movement of a thread is 'realer' and is better 'structured' than the movement of a line.

(4) The 'haziness' in the arc of the movement is most evident on a grey background and least evident on a black background. There seems to be a considerable qualitative or attributive difference between a movement experience evoked on a black ground as compared with one produced on a white ground. On black there is less of the attribute of 'greyness' and more of the attribute of 'protensity.' Moreover, it is the observation of *E* that there is very little 'flickering' or 'shimmering'[14] apparent in the case of movement of white on black, although something of the sort of thing described by the above words is noticeable in the case of movement of black on white.

IV. Influence of Encompassment

By drawing a line about an object we tend to mark it off from the rest of the total visual field. Our attention is drawn to it more easily and it stands out more clearly in consciousness. Even the space inside of a line or structure is more impressive and actually seems whiter.[15]

The experiment reported in this section concerns the influence of circumscribing the objective movement arrangement with a line.

Procedure. The following angle arrangements were used in this experiment:

Arrangement 1. Same as No. 2 in Fig. II.

Arrangement 2. Same angle arrangement, bounded by a circular line whose diameter was equal to the length of the lines in the angle arrangement.

Arrangement 3. Same arrangement as in 1, except bounded by a circle whose diameter was one half again as large as the length of the lines in the angle arrangement.

The instructions for Arrangements 1 and 2 were: "You will be shown a movement of a line. Immediately after a series of such movements you will be shown movement of a line within a circle. Compare the speed, clearness, completeness of movement, and haziness of the arc of the movement, in the case of movement of the line which is circumscribed, with movement of the line which is not circumscribed."

The *Os* were later given the same instructions for a comparison of Arrangements 1 and 3.

Because of lack of space it is necessary to omit the protocols.

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[14]Descriptive terms used by Dimmick's *Os*, *op. cit.*, 332.

[15]F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmungen: Einige Beobachtungen über die Zusammenfassung von Gesichtseindrücken zu Einheiten, *Zsch. f. Psychol.*, 23, 1900, 1-32, reported that the white space inside a figure is 'whiter' and more 'live' than that outside of the figure.

On practically all commercial moving picture curtains there is a dark border surrounding the limits of the frame of the picture for much the same reason.

Summary. (1) The protocols indicate that movement is more clearly perceived when surrounded by some fixed contiguous frame of reference. The ends of the lines carry the movement, and any addition to the total situation that tends to narrow the perceptual field and to accentuate the peripheral phase of rotary movement facilitates the general perception of the movement.[16] The effect produced is related to the effect produced by *Nebenreize* or *Nebeneindrücke* as reported in investigations of optical illusions; also, to 'pregnance of configuration'[17] as reported by configurationists. Under the conditions of this investigation, the movement of a line within a circle does not carry the movement over to the circle itself; that is to say, the line slides around inside of the circle. The circle itself does not move.

(2) Encompassing the angle arrangement by a larger circle, which is not actually contiguous to any part of the object which moves, also produces a clearer perceptual configuration of movement.[18] For reasons which seem indescribable, circumscription of the objective pattern of movement tends to bring the perceptual pattern of movement subjectively nearer and to render it more clear and complete. The effect is somewhat similar to that which one gets in framing a picture. A frame tends to compel the attention to the picture and causes it to stand out more definitely in consciousness. The limitation or bounding of the field in which something moves seems to assist the organism in limiting the field of consciousness, resulting in a higher degree of concentration upon the ground within the boundary, and, in the case of movement, producing a clearer and more impressive experience of movement.

V. Influence of Position

The object of this experiment was to ascertain the part played by the factor of position as a determinant in rotary movement. Everyday experience leads us to expect that it is just as easy to perceive rotary movement in one phase of its

[16]This seems to accord with the finding of T. Lipps, *Leitfaden der Psychologie*, 1909, 193, that when elements are used in a group they make less claim on perceptual activity (*Auffassungstätigkeit*), especially when they are in close union with other members of the group.

[17]W. Fuchs, Die totalisierende Gestaltauffassung, *Zsch. f. Psychol.*, 86, 1921, 8-16, shows that well known objects do not complete themselves unless they are 'pregnant' configurations. For instance, a straight line is not complete whereas a circle is. The head of a dog is not supplied in a drawing which lacks it. A line, forming a part of a good or 'pregnant' configuration like a star, is filled in providing it forms a necessary part of the configuration.

[18]Rubin, *op. cit.*, 130, throws some light on this question of 'structural clearness' in reference to contours.

total revolution as to perceive it in another phase. By varying the position of the lines in the angle arrangements and placing the movement experience under more critical descriptive conditions, lateral movement (*i.e.* on the sides) was contrasted with transverse or crosswise movement (*i.e.* above and below). More specifically, this procedure involved a study of the compulsoriness of the direction taken by movement starting from all positions in the circular field.

Procedure. Diametral arrangements as shown in the charts of Fig. V were presented to the *O*s in the order shown and in the reverse order. The method of limits was employed because it was the most convenient.

The *O*s were instructed: "Keep your eyes fixated steadily upon the point and observe and report the direction of the movement. Maintain, if possible, a neutral receptive attitude and in no way attempt to control the direction of the movement."

According to the results of the experiment, as presented in the Fig. V, rotary movement takes place more readily laterally than transversely.

Summary. (1) The results portrayed in the form of curves prove that the compulsoriness of rotary movement is not uniform at every position. The data represented in Fig. V demonstrate a decided preference on the part of the *O*s for lateral movement over transverse movement. This assumption was further corroborated through confrontation. The *O*s were shown the facts and asked for an opinion. Every *O* admitted that lateral movement was more compulsory than transverse movement. Furthermore, the *O*s noticed an inhibiting tendency in the vicinity of the vertical plane; that is to say, the direction of the movement tended to be reversed if the initial position of the line which moved was changed from right to left, or from left to right, of the vertical plane.

(2) Any explanation of this tendency seems to involve the question of general spatial level,[19] and of empathy or *Einfühlung*. Civilized man stands upright, is always aware to some degree of his vertical position and constantly and automatically adjusts for deviations therefrom. Likewise, as shown in sculpture and painting, we seem to have a tendency to judge figures and objects automatically with respect to their vertical position. The vertical position is the position of equilibrium. A line slightly off the vertical is potentially not in a position of equilibrium and by empathy we expect it to fall and regain a more stable position. In diametral movement the attention is nearly always directed upon that part of the line above the axis, and it

[19]Koffka, *op. cit.*, 572, points out that "the spatial level (*Raumlage*) has a marked tendency to remain constant, together with common directions of 'above', 'below', 'right' and 'left'." Again, *op. cit.*, 577: "We possess a general spatial level within which we are anchored." Cf. G. E. Müller, Ueber das Aubertsche Phänomen, *Zsch. f. Sinnesphysiol.*, 49, 1916, 109-244. Müller has made extensive investigations of the phenomenon.

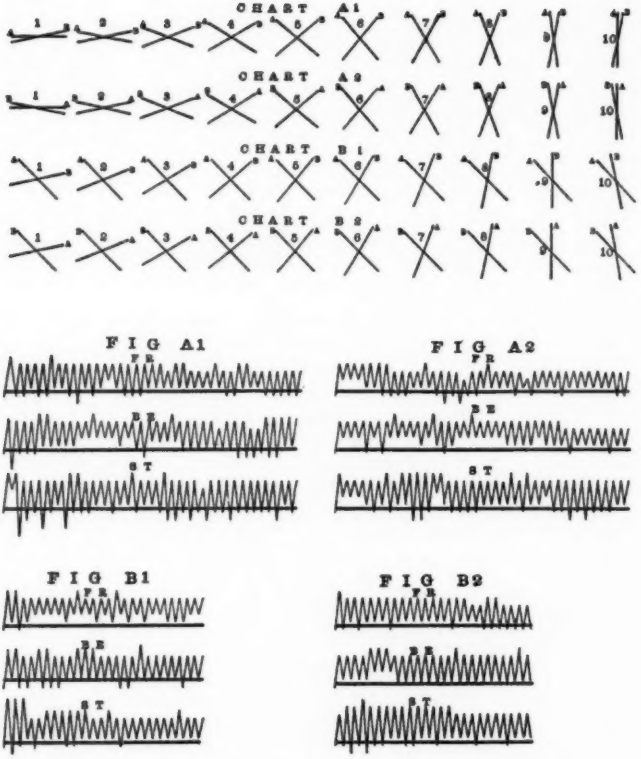

FIG. V. PREDOMINANCE OF LATERAL MOVEMENT OVER TRANSVERSE MOVEMENT.

The ordinates in Figs. A1, A2, B1, and B2 represent series of angle arrangements from 1 to 10 as illustrated respectively in Charts A1, A2, B1 and B2. The heavy horizontal line indicates the midpoint of the series, that is to say, angle arrangements 6 to 10 stand above the line while arrangements 1 to 5 stand below the line.

The abscissae represent exposures. Five exposures of each angle arrangement were given, beginning with No. 5 and proceeding upward until the limiting series was reached, then downward until the lower limiting series was reached, *etc.* The limiting series was that angle arrangement in which the movement, if clockwise, became counterclockwise, or, if counterclockwise, became clockwise.

In Charts A1 and B1 the arrangement of the series is such that the lateral movement of line A to line B is counterclockwise. In Figs. A1 and B1 the ascending lines indicate counterclockwise movement, the descending lines clockwise movement. The case is exactly the reverse with Figs. A2 and B2, that is to say, the lateral movement of line A to line B is clockwise, and the ascending lines in the Figs. A2 and B2 represent clockwise movement.

If position exerted no influence on the direction taken by the movement in the various series of angle arrangements, the limiting series on the ascending and descending lines of the Figs. might be expected to be equidistant from the heavy line. The uneven distribution of the curves above the heavy line is indicative of the significant influence of position.

is the upper part of the line which tends to fall when it is not in a state of vertical equilibrium.

VI. Influence of Fixation Point

One of the constants in all the experiments was the fixation point. Invariably it was made to coincide with the junction of the horizontal and upright lines. A new *O* in tachistoscopic observation always has difficulty in holding his gaze steadily and continuously upon the fixation point. Even with *O* trained in tachistoscopic observation one may question whether the artificial conditions attending the production of apparent movement may not cause him to vary his gaze ever so little. The only remedy for error of this kind, under the conditions of tachistoscopic experimentation, is to train the *O* to notice and report lapses and wanderings of fixation and attention. It is the belief of *E* that a trained *O* can maintain a relatively constant fixation when he is fresh, but that after a certain amount of repetition with resultant fatigue, a slight relaxing of attention is probable in spite of the best of good faith. If there is this occasional wandering of fixation to some other point or points in the stimulus field, we should endeavor to control it or at the very least determine what effect it has upon the movement experience. The data accumulated in this section were derived from a study of the stimulus and non-stimulus determinants of movement arising out of a variation of the objective fixation point.

Procedure and Results. Five fixation points besides the center point were used (see No. 8 in Fig. II), viz., center-left, upper-left, upper-center, upper-right, center-right. All of these fixation points were located along the circumference of a circle with a radius of the same length as the radial lines in the angle arrangements. The center-left and center-right fixation points were on a line with the center fixation point. The upper-center fixation point was directly above the center point, and the upper-right and upper-left points were at an angle of 45° with the horizontal.

A radial arrangement (No. 6, Fig. II) was adopted, consisting of a horizontal line 2 cm. long whose mid-point was coincident with a vertical line 1 cm. long.

The instructions were: "Throughout this experiment focus your gaze on the fixation point no matter where it appears. Try to keep your attention upon the point but notice and report the direction of the movement. Make every effort to hold your gaze upon the fixation point and report any lapses."

If at first the movement was clockwise down to the right then the upper-left fixation point was introduced. With the best efforts to fixate the new point, usually no change of direction was reported in the beginning. In this case 10 exposures of a left oblique radial angle arrangement (No. 1, Fig. II) were given. The direction of the movement was compulsorily counterclockwise and remained counterclockwise when the vertical radial arrangement was again inserted.

At this stage, the upper-right fixation point was inserted in an effort to reverse the direction of the movement. If reversal took place, the upper-left fixation point was again inserted and alternated regularly with the

upper-right fixation point, until the direction of the movement changed automatically with the change of the fixation point. If the reversal did not occur, a right oblique angle arrangement (dotted line of No. 1, Fig. II) was introduced intermittently to bring about a clockwise set.

Repetition of the above procedure was continued until eventually the upper-right and upper-left fixation points came to be in themselves sufficient to reverse the direction of the movement in the equivocal situation.[20] Occasionally, after the first day, an oblique arrangement had to be introduced to reverse the direction of the movement. However, having once trained an *O* to attend properly, the fixation point proved adequate to vary the direction of the movement.

The same procedure was followed for the center-right and center-left fixation points.

Finally, after the *O*s were well trained in the use of variable fixation points, they were asked to compare the effectiveness of the upper-right and upper-left fixation points with the effectiveness of the center-right and center-left fixation points, in rendering the direction of the movement compulsory. In every case the *O*s reported that the center-right and the center-left fixation points were more effective in bringing about an immediate and clear reversal of movement than the upper-right and upper-left fixation points. The reason obviously seems to be that the movement is more clearly perceived when one fixates the point where the movement is completed. Upon confrontation with the data the *O*s corroborated the above supposition.

As a further check upon this conjecture the vertical angle arrangement was presented in reverse order, the horizontal line first and the vertical radius second (cf. No. 6, Fig. II). The same general procedure used for the first radial arrangement was followed, and an endeavor made to determine the relative effectiveness of the five circumferential fixation points shown in diagram 8, Fig. II, in producing clear movement from the equivocal arrangement above. Invariably, the upper-center fixation point was declared to be the most effective in producing clear movement. In the latter case such phrases as "snaps into position", "comes up with a crash", were used in describing the movement. When the center-left and center-right fixation points were utilized it was easy to perceive the start of the movement, but the completion of the movement was extremely vague. Fixation upon the upper-left and upper-right fixation points enabled the *O*s to grasp the whole of the movement fairly well but the end of the movement was found to be vague. Clear perception of the conclusion of the movement seems to be an important factor in 'structuring' of the movement pattern. Fixation upon the point of final rest seems to correspond to what Wertheimer calls the *Schwerpunkt*, and proves to be the most effective of the circumferential points in the production of clear movement. The beginning of the movement is *aufdringlich*, that is to say, it compels the attention naturally, but if the *O* is not able to perceive clearly the fading out of the movement, which is not so spontaneously obtrusive, he is unable to grasp the movement so well as a totality.

This general principle holds only in the case of rotary movement through arcs of 90° or less. It does not hold so clearly in the case of rotary movement through an arc of more than 90°. Movement through obtuse angles is more clearly perceived if the intermediate fixation points (upper-right and upper-left) are used, because if the gaze is directed upon the center-right fixation point in an obtuse angle arrangement the central end of the left oblique line often slides out sideways to the right instead of remaining in place and acting as an axis.

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[20] Koffka, *op. cit.*, 578, refers to a change of this kind as a change of points of anchorage (*Verankerungspunkte*).

A comparison of the effectiveness of the center fixation point with the peripheral fixation points is in favor of the center point except in the case where the peripheral point is located at, or very near to, the point where the movement is completed. Movement is carried by the outer ends of lines and the peripheral position where the movement is completed seems to be the most effective point of regard for the perception of clearest movement. The center fixation point is more effective than a point at the beginning or at intermediate positions on the peripheral arc of movement. There is, after all, no great difference in the relative effectiveness of the center fixation point and the peripheral point which coincides with the completion of the movement, but the advantage seems to be with the peripheral point.[21]

No effort was made to test out, with reference to linear direct movement, the principle that movement is most clearly perceived when one fixates the point where the movement is completed. However, the facts of this experiment indicate that the principle might hold equally well in the case of any other kind of movement.

Summary. It was found that change of the *O*'s fixation point varies significantly the gross perception of movement, *i.e.* movement seems to appear better where there is the greatest concentration of attention, but, owing to the fact that the starting position of a presentation of movement is the most impressive position for the human organism, the most favorable position of fixation for perceiving a presentation of movement as a whole is at or near the position of completion of the movement.

VII. Influence of Form of Stimulus on Direction of Movement

Although movement perceived under tachistoscopic conditions for optimal time is practically compulsory, the direction taken by the movement is not always compulsory. There are equivocal situations in which the direction of movement seems to be wholly a matter of chance. For instance, in the vertical radial angle arrangement (No. 6, Fig. II) the line 'A' appearing first and followed by line 'BB,' sometimes revolves clockwise, and sometimes counterclockwise, to 'BB.'

This arrangement was presented to the *O*s at the beginning of an experimental hour on 12 different occasions in order to determine if possible their natural preference for one direction over the other direction. The only instructions were: "Report the direction taken by the movement."

	ST	BE	FR
Clockwise movement	9	4	5
Counterclockwise movement	3	8	7

[21]This conclusion corroborates the statement of Wertheimer (*op. cit.*, 207) to the effect that movements are not due to processes at the periphery of attention, but that movements appear better where there is the greatest concentration of attention. However, these data controvert the statement made by Wertheimer that "the nearer attention is to the apex of the angle the better the movement" (*op. cit.*, 207).

These figures indicate the number of times the direction of the movement was reported clockwise or counterclockwise, without the *Os* being influenced in any way (as far as one could tell without attempting to control the directional pre-perception) by a rotary movement perception contiguous in time, that might set up a directional predisposition.

The results are equivocal. No definite tendency, either clockwise or counterclockwise, is apparent.

A variation of the above experiment consisted in the use of a vertical diametral arrangement (No. 7, Fig. II).

The procedure was the same and the results are presented below.

	ST	BE	FR
Clockwise movement	5	3	1
Counterclockwise movement	7	9	11

A very definite counterclockwise tendency is exhibited in a movement evoked by the diametral arrangement, a finding which corroborates results in Section II.[22] No definite *Aufgabe* was used in this preliminary experiment and the varied results are probably due to the variation of uncontrolled attitudes. It is interesting to note that movement of diameters is less subject to this variation than movement of radii, a fact which supports similar conclusions obtained under different conditions, as reported below in Section IX.[23]

Using these preliminary experiments as a starting point the following more systematic investigation was formulated in an effort to determine the influence of any permanent directional predispositions.

Procedure. The instructions were: "Observe the movement reporting the direction it takes, or, in case of 'splitting' report, 'split.' Maintain a neutral receptive attitude if you can, neither facilitating nor hindering the movement experience. Report any occasion in which you find yourself disobeying directions." The *Os* experienced little difficulty in adopting the receptive attitude and reported few cases of disobedience.

The results are shown in Tables II and III, where the nature of the stimuli is also sufficiently indicated. The vertical stimulus was presented first in Parts 1, 2, and 5, and the horizontal stimulus first in Parts 3 and 4.

The seen movement was clockwise, counterwise, or 'splitting,' *i.e.* the line split for the equivocal stimulus and moved in both directions, or at least there was movement and the direction of movement was equivocal. Where the two columns of Tables II and III do not total 25, it is to be understood that the remainder of the 25 cases were 'splitting' movements.

A clockwise or counterclockwise predisposition was established by a continuous preliminary series of 10 exposures in which the angle was reduced by 10° in the direction in which predisposition was desired. Thus a clockwise predisposition was set up by preceding stimulus No. 6, Fig. II, by a similar stimulus in which the vertical line made an angle of 80° at the right and 100° at the left (instead of 90° in both cases) with the horizontal line. Movement through an acute angle, even at 80°, is compulsory as against the complementary obtuse angle.

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[22] 481 ff. [23] 497 ff.

TABLE II. MOVEMENT FOR AN EQUIVOCAL STIMULUS AS DEPENDENT UPON PREDISPOSITION. For nature of equivocal stimuli see 6, 7, and 3 of Fig. II. Predisposition established by 10 preliminary exposures, 10° more acute than equivocal angle (movement is compulsory through an acute angle as against an obtuse). C = clockwise seen movement. CC = counterclockwise movement. 25 cases in each set. Where the C and CC columns do not total 25, the remainder were 'splitting' movements (both directions). The 10 pairs of figures in each block of the table represents 10 experiments of 25 cases each under the same conditions, with predisposition established anew in each of the 10 experiments.

Observer		ST						BE						FR					
Stimulus No. See Fig. II		No. 6		No. 7		No. 3		No. 6		No. 7		No. 3		No. 6		No. 7		No. 3	
Direction of seen movement		CC	C	CC	C	CC	C	CC	C	CC	C	CC	C	CC	C	CC	C	CC	C
Part 1	Clockwise predisposition Vert. stim. precedes horiz.	13	12	0	25	0	25	5	10	5	20	0	25	16	5	0	25	0	25
		18	7	0	25	0	25	7	13	5	15	0	25	10	10	0	25	0	25
		10	15	5	20	0	25	10	12	13	7	0	25	8	5	23	2	0	25
		11	14	3	22	0	25	2	18	3	15	0	25	6	2	0	25	0	25
		7	18	0	25	0	25	10	10	7	10	0	25	0	2	0	25	0	25
		23	2	0	25	0	25	2	15	10	10	0	25	0	18	0	25	0	25
		10	15	0	25	0	25	10	13	2	20	0	25	0	15	0	25	0	25
		16	9	0	25	0	25	8	14	0	24	0	25	0	17	0	25	0	25
		13	3	2	23	0	25	15	10	13	5	0	25	2	14	2	20	0	25
		11	14	1	24	0	25	8	15	0	25	0	25	10	5	0	25	0	25
Part 2	Counterclockwise predisposition Vert. stim. precedes horiz.	10	15	25	0	25	0	12	5	25	0	25	0	25	0	25	0	25	0
		8	17	14	11	25	0	20	0	21	0	25	0	15	0	25	0	25	0
		11	14	18	7	25	0	15	0	12	8	25	0	20	0	25	0	25	0
		12	6	10	15	25	0	17	8	23	0	25	0	3	0	20	0	25	0
		7	13	5	20	25	0	15	0	25	0	25	0	22	0	25	0	25	0
		8	15	25	0	25	0	18	2	20	0	25	0	15	5	20	0	25	0
		7	5	7	18	25	0	16	6	25	0	25	0	21	0	22	0	25	0
		8	5	25	0	25	0	12	8	18	5	25	0	20	0	25	0	25	0
		10	6	17	8	25	0	16	0	15	10	25	0	24	0	21	0	25	0
		17	5	14	11	25	0	10	4	19		25	0	25	0	17	0	25	0
Part 3	Clockwise predisposition Horiz. stim. precedes vert.	11	14	12	13	0	25	12	8	13	12	0	25	0	20	0	25	0	25
		10	15	15	10	0	25	15	5	8	7	0	25	0	15	0	25	0	25
		8	17	0	25	0	25	20	0	12	5	0	25	0	3	0	25	0	25
		10	15	15	10	0	25	18	2	20	0	0	25	0	20	0	25	0	25
		5	20	5	20	0	25	0	5	0	25	0	25	0	10	0	25	0	25
		7	8	0	25	0	25	0	4	0	25	0	25	0	13	0	25	0	25
		8	15	9	16	0	25	0	2	0	25	0	25	0	5	0	25	0	25
		4	18	0	25	0	25	5	13	2	18	0	25	0	12	0	25	0	25
		0	20	7	18	0	25	2	20	7	15	0	25	0	17	0	25	0	25
		11	14	2	23	0	25	0	17	10	10	0	25	0	20	0	25	0	25
Part 4	Counterclockwise predisposition Horiz. stim. precedes vert.	20	5	13	12	25	0	18	3	15	10	25	0	5	0	25	0	25	0
		10	15	25	0	25	0	20	0	25	0	25	0	2	0	25	0	25	0
		18	7	25	0	25	0	18	2	5	8	25	0	2	0	25	0	25	0
		14	11	25	0	25	0	20	0	25	0	25	0	10	0	25	0	25	0
		23	2	25	0	25	0	3	0	25	0	25	0	2	0	25	0	25	0
		20	5	25	0	25	0	4	1	23	0	25	0	17	0	14	0	25	0
		12	13	25	0	25	0	2	0	20	0	25	0	10	0	20	0	25	0
		10	15	25	0	25	0	12	0	18	2	25	0	14	0	18	0	25	0
		19	6	25	0	25	0	16	2	17	0	25	0	13	2	25	0	25	0
		17	8	25	0	25	0	19	0	20	0	25	0	20	0	20	0	25	0

TABLE III. MOVEMENT FOR AN EQUIVOCAL STIMULUS AS DEPENDENT UPON PREDISPOSITION. Same as Table II, *q. v.*, except that stimulus is two paddles of No. 4, Fig. II, arranged as in No. 7, Fig. II.

Observer		ST		BE		FR	
Direction of seen movement		CC	C	CC	C	CC	C
Part 5	Clockwise predisposition Vert. stim. precedes horiz.	0	25	0	25	0	25
		0	25	0	25	0	25
		0	25	0	25	0	25
		0	25	0	25	0	25
		0	25	0	25	0	25
		0	25	0	25	0	25
		0	25	0	25	0	25
		0	25	0	25	0	25

Summary. (1) The tendency for a line to move counter-clockwise in the equivocal situation (where the first appearing line may move in either of two directions) is much more marked than the tendency for it to move clockwise. Even a set of 10 exposures of a clockwise movement does not seem to have any considerable influence in facilitating the clockwise movement in the repetition of an equivocal angle arrangement where the direction taken by the movement is in no way controlled by the objective stimulus.

(2) The direction of the movement of a vertical radius to a horizontal diameter, or of a horizontal diameter to a vertical radius, was found to be the least compulsory and the least determinate of all movements. This particular arrangement is so simple that apparently the slightest variation in attitude, eye-movement or fixation reverses the direction of the movement or else evokes a 'splitting' phenomenon.

(3) The direction of movement of one diameter is more compulsory than the movement of one radius. Two movements, one on each side of the axis, of course are involved in the case of diametral movement, but nevertheless, splitting and uncertainty sometimes occur. The movement of this arrangement is much more determinate and the directional tendency much more compulsory, yet attitude and variation of attention are able to exercise an influence in spite of the best efforts on the part of the *O*.

(4) The direction of movement of two diameters at right angles to each other is more compulsory than the movement of one diameter. In no case was there a variation from the direction initiated by the predisposition. The *O*s remarked on the fact that the movement of two diameters had a 'compulsory' character to it. Later on, after the regular experiment was concluded, the *O*s were given these angle arrangements and asked to alternate voluntarily the direction of the movement. After a considerable amount of practice, they were able to predetermine the direction of the movement, but only with the greatest difficulty and strain. Voluntary predetermination of direction was found to be much more difficult in the case of movement of two diameters than in the case of movement of one diameter.

(5) Table III brings out the fact that thickening the ends of the lines stabilizes the directional tendency of movement. Increasing the relative size of the ends of the lines seems to result in discounting the effects of attitudes, fatigue and variation of attention, and makes it more difficult for the *O* to control and vary the direction of movement. As was observed in an earlier section, the ends of rotating lines carry the movement and therefore thickening of the ends of the lines makes the

movement more definite, clear-cut, 'intense', and probably more meaningful due to the similarity to a windmill. Once started, such a movement seems to 'stamp in' the perceptual directional configuration.

(6) It would seem therefore, that the stimulus pattern is closely correlated with the subjective configuration, and that the form, complexity and meaningfulness of the stimulus may and often do determine the quality as well as the compulsoriness of the direction of the movement perception. One *O* reported that, when he set himself very conscientiously to his task, 'splitting' movements tended to be eliminated. The reversals of direction of movement came suddenly, but the degree of compulsoriness seemed to vary from time to time with change of attitude.

VIII. Influence of Splitting

The use of an equivocal situation (No. 6 in Fig. II), in which the movement may go in either a clockwise or counterclockwise direction, sometimes gives the unique experience where the movement splits and goes fanwise in two directions. The object of the experiment of this section was to analyze and describe this experience.

Procedure. Radial and diametral arrangements as represented in Nos. 6 and 7, Fig. II, were presented to the *O*s repeatedly. The instructions were as follows: "Designate the direction of the movement, or, in case of splitting, report 'split'." Whenever an *O* reported 'split' he was requested to explain as well as he could the nature of the splitting movement.

Owing to limits of space it is necessary to omit the protocols, but the following summary is based on explicit reports.

Summary. (1) Splitting movements are due to a variety of factors, both subjective and objective (*subjective:* variation of attitude, wandering of attention, variation of focus and distribution of attention, 'trying' to break up the existent directional tendency, fatigue, relaxation of visual accommodation; *objective:* reduction of the complexity and meaningfulness of the vehicle of movement, and, of course, an equivocal situation).

(2) This examination of splitting movements brings to light a multiplicity of determinants. The description of the splitting movement by the *O*s bears a remarkable resemblance to 'bare,' 'objectless' movement, that ultimate unit of visual experience denominated by Wertheimer as the 'pure' *phi*-phenomenon. However, unlike the 'pure' *phi*-phenomenon of Wertheimer, splitting perceptions were found to depend upon proper experimental conditions and also to be controlled to a considerable extent by *Aufgaben*. Subjectively, the splitting movement seemed to be brought to the fore when the image of the first line of the arrangement persisted in consciousness and when attention was taken from the 'reality' of the vehicle of movement and the ground against which it moved.

(3) To throw more light on this question, *E* set himself to observe the movement of acute angles, under varying conditions of visual accommodation. He believes that he developed the ability, after much practice, to experience the movement while accommodating to far vision, and to compare it with the experience of the movement when accommodating to near vision. In the case of accommodation to distance, the movement appeared vague and 'flashy.' A phenomenological description of the movement under such conditions would approximate to the description of Wertheimer's 'pure' *phi*-phenomenon, as movement divorced from anything that moved.[24] Such a statement hardly makes sense, but phenomenologically something of the sort is what one occasionally experiences.

While accommodating for the object which moved and forcing the attention upon the existential situation, *E* was unable to experience anything which subjectively corresponded to 'pure' *phi*-movement. However, by accommodating to distance vision, turning the attention away from the existential situation, and assuming a passive receptive attitude, he was able to experience something resembling pure 'flashiness' or 'flashy filminess' with little other accompanying context.

In sum therefore, splitting movements are vague movements, perhaps partly unlocalized in space, dependent largely upon and controlled to a considerable extent by fatigue and other subjective factors. Moreover, it seems to *E*, that there is a remarkable similarity between the description of the splitting movements observed in this experiment and the 'pure' *phi*-phenomenon observed and reported by Wertheimer's *O*s. The results indicate that further investigation might yield further determinants which would enable us to predict and control all 'pure' *phi*-movements.

Because of its unique nature it is suggested that 'splitting movement' be considered as a separate kind of movement in any future classification of varieties of visual movement.

IX. Influence of Voluntary Predispositions on the Direction Taken by the Movement

It was the purpose of the experiment of this section to investigate the dependence of the perception of the direction of movement upon conditions not determined by the stimulus, that is to say, the extent to which the *O* has control over the direction taken by apparent movement.

Procedure A. A left oblique angle arrangement (No. 1, Fig. II) was shown, and *O* requested to alternate the movement, now counterclockwise, now clockwise, now counterclockwise, now clockwise, etc. He was directed

[24] Wertheimer, *op. cit.*, 213.

to use any method whatsoever in his effort to control the direction of the movement. If *O* was unable to make the movement carry across the obtuse angle to the right, an oblique angle arrangement with an acute angle to the right (dotted line, No. 1, Fig. II) was introduced in order to facilitate the establishment of a clockwise directional tendency. As these two angle arrangements at first evoked compulsory movement in opposite directions, they were exhibited to *O* in whatever order he chose in order to assist him in his effort to overcome the directional compulsoriness of the stimulus, and thereby to aid him to achieve voluntary control over the direction of the movement across an obtuse angle. After a surprisingly short time, all *O*s found themselves able to alternate at will the direction of the movement, now over the obtuse angle, now over the acute angle, etc.

Procedure B. Besides the radial arrangements, the *O*s were shown diametral arrangements (No. 2, Fig. II), double diameter arrangements (No. 3, Fig. II) and 'windmill' arrangements (No. 4, Fig. II).

The instructions were: "Compare and report the differences, if any, in the ease or facility with which you find yourself able to control the direction of the movement in the different arrangements."

The following summary is based upon explicit protocols.

Summary. (1) Voluntary control of the direction taken by rotary movement is easiest in the case of radial and diametral arrangements. The movement of two diameters, and of two diameters thickened at the ends, is much more difficult to control. The 'bareness' and simplicity of the movement of a radius or of a diameter lends itself more readily to change of direction. The movement of two diameters and of a 'windmill' is much more complicated and involves many more phases of movement. The 'windmill' movement has more of a 'body' or 'thing' character to it. The bare lines have more of a 'film'[25] character and are not so attention-compelling. The 'windmill' movement is not so much 'a movement' as 'the movement of a windmill,' or 'the movement of thick lines.' Bare lines seem unmeaningful in much the same way that nonsense syllables are unmeaningful. We are aware of the lines and aware that they move, but it is not exactly the same as if something real moved. In the case of the 'windmill' arrangement the movement is indubitably 'realer.' The movement attracts the attention more naturally just as a real word may attract the attention more effectively than a nonsense syllable. The nature of the influence of meaningfulness on the phenomenon of apparent movement is very difficult to describe, yet there is something here quite obvious and quite real to the *O*. It resists verbal analysis and is very difficult to approach, because it concerns the core of psychological 'meaning,' which presents as yet much mystery to experimental psychology.

(2) The *O*s reported voluntary predetermination of movement to be very fatiguing. An actual physical strain is involved.

[25]Resembling the *Flächenfarbe* described by D. Katz, Die Erscheinungsweisen der Farben, *Zsch. f. Psychol.*, Erg. Bd. 7, 1911, 6-12.

All the forces of *O* seem focussed upon the problem and the fatigue is a muscular fatigue of the entire organism. This voluntary alternation of movement at first seems to involve the 'will' or voluntary power of *O* in a more complete degree than is necessary in ordinary receptive observation. It involves a conscious psychosomatic straining on the part of *O*. This experience and the experience engendered by the ordinary receptive neutral attitude seem to be just as wide apart attitudinally, as the experience of a man who is creating something (book, poem, painting), and the experience of a man who is merely reading a book or absorbing casual information of any sort.

(3) Kinesthesis in eyeballs, face, neck and head as accompaniments of movements seems to vary considerably among different *O*s. These kinesthetic components of voluntary effort are among the conditioning factors of movement, but they are not constant nor found present with all *O*s, and therefore are not necessarily to be regarded as the outstanding conditioning factors.

(4) Our regular *O*s and four other casual *O*s who served in this experiment found counterclockwise movement more compulsory than clockwise movement. All *O*s were right-eyed and right-handed.

(5) The question of right-handedness and right-eyedness seemed an interesting cue and two left-handed students were induced to observe. Experimentation with equivocal situations demonstrated a preference for movement in a clockwise direction. In other words, the directional compulsoriness of the movement in the case of left-handed *O*s was found to be exactly the reverse of that in right-handed *O*s, a discovery which indicates that the directional compulsoriness of clockwise movement or of counterclockwise movement may be due to 'handedness' or 'eyedness.'[26] These facts also accord with some of the findings of Dallenbach[27] and others[28] in reference to 'position' as a determinant of consciousness. Their results indicate that "specific preferential position seems to be a matter of right- or

[26]B. S. Parsons, *Lefthandedness*, 1924, 1-85, summarizes the data on the subject, including experiments of his own, and concludes that handedness is dependent on eyedness.

[27]K. M. Dallenbach, Position vs. Intensity as a Determinant of Clearness, this JOURNAL, 34, 1923, 282-286.

[28]L. G. Meads, Form vs. Intensity as a Determinant of Attention, this JOURNAL, 26, 1915, 150-151; J. N. Curtis and W. S. Foster, Size vs. Intensity as a Determinant of Attention, *ibid.*, 28, 1917, 293-296; A. M. Bowman, Size vs. Intensity as a Determinant of Attention *ibid*, 31, 1920, 87-90; Dewey and Dallenbach, *ibid*, 35, 1924, 121-125; Burke and Dallenbach, Position vs. Intensity as a Determinant of Attention of Left-handed Observers, *ibid*, 35, 1924, 267-269.

left-handedness; the positions to the left and above have a distinct attentional advantage for right-handed *O*s and contrariwise the positions to the right and below have an attentional advantage for left-handed *O*s."[29]

In sum, the part played by attitudinal variables in the direction taken by movement is very significant. However, *E* maintains that the direction of movement in an equivocal situation is not merely a matter of chance or ignorance, but rather depends upon certain determinate or determinable conditions, many of which were mentioned in this Section and in Section VII.

Conclusions

Some determinants of the attributive characteristics (such as duration, greyness, clearness, rapidity, completeness) of the phenomenon of apparent movement may be listed as follows:

A. Determinants not in the stimulus:

(1) *Anayltic vs. synthetic attitude.* Laboratory 'atmosphere' is favorable for an analytic attitude. A synthetic attitude develops with habituation and training. Decay of one attitude results in growth of the other (473, 479).

(2) *Passive-receptive vs. active-striving attitude.* When *O*s are habituated to the experiment and a synthetic attitude has developed, a passive, receptive attitude has been found to favor a uniform distribution of attention over the stimulus field and hence a more nearly univocal perception of movement. An active, striving attitude under conditions of training tends to merge into an analytic attitude and to bring about a perception of succession rather than of movement (479, 498).

(3) *Attentional preference for rotary movement in certain directions.* Rotary movement seems to be initiated more readily in a counterclockwise direction from the vertical position, at least in the case of right-eyed *O*s (498).

(4) *Position.* There seems to be an inertia of the organism against carrying rotary movement across the vertical axis. In other words, rotary apparent movement takes place more readily laterally than transversely (487 ff.).

B. Determinants in the stimulus:

(1) *Distractions and artificiality of tachistoscopic technique.* Except in the case of the well-practiced *O*, it is found that an even distribution of attention and a uniform maintenance of gaze upon the fixation point is rendered particularly difficult in tachistoscopic observation owing to the diminutive size of the stimulus field, to light changes and to noises (489).

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[29]Burke and Dallenbach, *op. cit.*, 269.

(2) *Background.* Relative variation of stimulus and background influences the clearness, speed and haziness of the perception of movement. Movement of white on black gives the clearest and sharpest perception, black on white the next, and black on grey the poorest (481).

(3) *Fixation point.* A peripheral position at or near the completion of rotary movement (which takes place within the limits of the fovea) is the most effective point for fixation (490 f.).

(4) *Form of the vehicle of movement.* In general, an increase in the complexity of the object favors good movement. The perception of movement of a solid object is clearer than the perception of movement of a unidimensional object. Lines which are thickened at the ends revolve more easily and definitely than lines of uniform width. The direction of the movement of a double-diameter (No. 3, Fig. II) is less subject to voluntary change than the direction of movement of a single diameter (No. 7, Fig. II) (480, 494).

(5) *Circumscription.* Surrounding the stimulus field by a line increases the clearness of the perception of movement (486).

(6) *Meaningfulness.* In general, a meaningful, animate pattern gives a clearer perception of movement than an unmeaningful, inanimate pattern (474, 480).

(7) *Splitting movement.* Perception of an equivocal situation (No. 6, Fig. II) often gives rise to a 'splitting' movement, which presumably corresponds to Wertheimer's 'pure' *phi.* Splitting movement seems to be dependent upon definite conditions and is not a matter of chance (495 f.).

(8) *Repetition.* With repetition of the stimulus, the movement pattern at first improves and then, with the development of fatigue, gradually decays. The decay of the movement pattern of animate objects is more gradual than the decay of the movement pattern of inanimate objects, and of objects structurally more simple (479 f.).

A hitherto neglected factor in the phenomenon of apparent movement is the basis for the directional tendency, *i.e.* the cause of the direction of the movement in an equivocal situation. (The 'equivocal situation' is a figure in which an object may go in either of two directions; cf. No. 6, Fig. II.) The direction taken by the movement in such an arrangement has been found to depend upon some of the determinants listed above.

It is a theoretical implication of this investigation that the so-called *phi*-phenomenon is not an invariable and uncontrollable experience. The perception of movement is a complex, variable

experience, not necessarily analyzable into sensational units, but certainly analyzable into conditions responsible for its phenomenological structure. It is partly controllable, and is subject to a multiplicity of determinants, some of which have been discovered in the course of this investigation and are described in this article.

The position of the *Gestalt* psychologists implies that some sort of constancy exists between the physical configuration and the phenomenal structure. The results of this investigation accord roughly with such a conception, while at the same time they emphasize the tremendous importance of subjective determinants. Even the univocality or discreteness of the perception of movement seems to be a relative affair, depending not only upon determinants in the stimulus, but also depending to a considerable extent upon vague 'natural' or 'artificial' attitudinal variables which call for further study.

MESCAL VISIONS AND EIDETIC VISION

By Heinrich Klüver, University of Minnesota

It has been stated[1] that the discovery of the eidetic disposition ranks first among "the true advances of psychology in the last decade." Unfortunately, the investigator of eidetic phenomena who is not eidetically gifted is unable to observe the behavior of *Anschauungsbilder* for himself. Consequently the interpretation of certain statements repeated again and again by his eidetic subjects involves some difficulties. Now, according to W. Jaensch (**13**) *Anhalonium Lewinii* provides the possibility "bei Nichteidetikern Anschauungsbilder . . . . zu erzeugen." So the writer took mescal buttons (peyote)—kindly provided by Professor S. W. Fernberger—with the object of producing experimentally phenomena that are, or are comparable to, eidetic images. The question was whether some of the characteristics of eidetic images as determined by experimental work in and outside of Marburg can also be found in mescal vision.[2] In the following account attention is paid both to the similarities and to some striking differences between mescal vision and eidetic vision. At the same time observations not bearing directly on eidetic problems will be discussed.

For purposes of investigation peyote has been used quite frequently. The peyote cult of many Indian tribes (see bibliography) gave impetus to a number of researches upon the physiological effects of the drug. Psychologists were, aside from obtaining descriptions of mescal visions, interested in sensorimotor changes, in the influence upon higher thought processes, and in changes of personality. Very infrequently, however, mescal experiments have been considered as means to elucidate more general questions of the psychology and pathology of perception. Here Knauer and Maloney (**16**) must be mentioned for their attempt to throw light upon "the mechanism of visual hallucinations" by means of experiments of this kind.

The following experiment was conducted in the Psychological Laboratory of the University of Minnesota on Oct. 18, 1925. I wish to express my thanks to Mr. C. Jacobsen for his help in experimenting and in taking down my observations. Practically the whole time between 11 a. m., (Oct. 18), and 2:30 a. m., Oct. 19, was spent in one of the rooms of the Psychological

[1] O. Klemm, *Arch. f. d. ges. Psychol.*, 47, 1924, 223.

[2] Compare especially: Jaensch, E. R. (**12**). See also: (**15**).

Laboratory. After that I went home and slept restfully from about 3 a. m. to 11 a. m., Oct. 19. (As to dreams during this time, I recall only the fact that I was discussing a rather abstract philosophical problem with somebody.) The buttons were not chewed and swallowed in the Indian fashion, but 43 grm. were ground to powder. At 11 a. m. I put 13 grm. of this powder in a glass of water and drank it. Even taken in this way it was very nauseating. At 11:40 a second dose of 10 grm. was taken. I placed portions of teaspoon size on my tongue and washed them down with water. After this second dose I felt rather 'seasick.' At 12:20 I vomited. Soon hereafter the visions began. Aside from a slight frontal headache, dryness in the mouth and an occasional pressure in the stomach, I did not experience any bodily discomfort throughout the rest of the day. Since I was afraid that the 23 grm. of the first two doses would not be effective enough because of the vomiting, I thought of taking a third and even a fourth dose; but because this idea invariably caused nausea I limited myself to the previously described doses.

Description of Mescal Visions

The resulting phenomena were observed with closed eyes (blinders) unless special mention is made of the fact that the eyes were open. It may be remarked that I myself am neither *Eidetiker* nor do I possess vivid and complete optical memory-images of objects.

Concerning the visions obtained in this experiment the following general observations should be noted:

(1) Adequate descriptive terms are difficult to find. Most investigators emphasize the point that the phenomena defy all description. H. Ellis says, "The chief character of the visions is their indescribableness" (6). This is due partly to the fact that forms and colors show certain characteristics not experienced previously in such a way, and partly to the transitoriness of the phenomena.

(2) The phenomena are frequently but not always localized at reading distance. Sometimes they are nearer or farther away. Movements may occur in all directions, but movements of approach and recession do not occur so often.

(3) The center of the visual field is often distinguished from the remainder of the field; there may be greater brightness, a different color or rotation while the remainder of the field is motionless (or vice versa). In the case of designs the center is often aesthetically the dominating part. Frequently, the field of vision is similar to the interior of a cone the vertex of which is lying in the center of the field directly before the eyes (or vice versa).

(4) The visions cannot be influenced by 'thinking' and 'will.'

(5) Visual memory-images and visionary phenomena exist independently.

(6) All spectral colors can be observed. There is no indication that certain colors occur more frequently than others. Sometimes approximately complementary colors appear simultaneously or successively.[3]

(7) In the majority of cases the arrangement of objects in the optical field is symmetrical.

(8) The observation of the phenomena is accompanied by pleasant feeling tones.

As to the following description, it must be remembered that until 2:30 a. m., Oct. 19, upon closing the eyes visions could always be seen. Taking into consideration the rapid changes it is clear that only comparatively few observations could be recorded.

Protocol transcribed by Mr. Jacobsen.

12:30 noon. Clouds from left to right through optical field. Tail of a pheasant (in center of field) turns into bright yellow star; star into sparks. Moving scintillating screw; 'hundreds' of screws. A sequence of rapidly changing objects in agreeable colors. A rotating wheel (diameter about 1 cm.) in the center of a silvery ground. Suddenly in the wheel a picture of God as represented in old Christian paintings.—Intention to see a homogeneous dark field of vision: red and green shoes appear. Most phenomena much nearer than reading distance.

12:35. The upper part of the body of a man, with a pale face but red cheeks, rising slowly from below. The face is unknown to me.—While I am thinking of a friend (visual memory-image) the head of an Indian appears.

12:45. Wavy spongy ground of greenish black color.—Glowing coals turn into pin heads of brilliant white color.

1:05 p. m. Wonderfully cut deep red jewels constantly changing in size, shape, color and distance from each other. Sometimes covered with silvery or greenish drops. The whole very symmetrical. The design on a background the color of which changes frequently.—Try to see the experimenter, but: blue clouds, a church, the dim face of an old man on background, blue arabesques. The whole seen through a steel veil the meshes of which are constantly changing in size and form.

1:10. Bright yellowish sparks irregularly distributed in the whole field.

1:15. Beads in different colors. Colors always changing: red to violet, green to bright gray, etc. Color so bright that I doubt that the eyes are closed.—Yellow mass like saltwater taffy pierced by two teeth (about 6 cm. in length).

1:20. Eyes open: the pupils alternately dilate and contract in day light; difficulties in focussing objects. Closing the eye: arches in violet. Black quatrefoils with three corners. The fourth corner seems to be lacking. Impossible to 'get' the fourth foil: the quatrefoils begin moving to the

[3]Compare, however, W. Nägeli, Über selbstbeobachtete Gesichtserscheinungen, *Sitz. Ber. d. K. Bayer. Akad. d. Wissensch.*, 1, 1868.

left and disappear.—Silvery water pouring downward, suddenly flowing upward.—Landscape as on Japanese pictures: a picture rather than a real landscape.—Sparks having the appearance of exploding shells turn into strange flowers which remind me of poppies in California.

1:25. Again the quatrefoil. Cannot get the fourth foil.

1:30. Open the eyes for a few seconds, look at the experimenter and close them: positive after-image of the face of the experimenter. His hair turns into hair of a cat, his eyes get a bright yellowish color. Then the head of a cat (natural size); hereafter the whole field filled with yellow eyes. (No change in size of the eyes during the modifications.)—Then a brilliant blue wheel rotating in left upper field. Blue wheel turns into red sparks.

1:35. Spiral rapidly revolving in center of field drawn out away from me.—(Blinders taken off, eyes remain closed): field turns red, red jewel motive appears; jewels now very small (diameter perhaps .3 mm.).—Elephant (size as on picture) turned upside down.

1:45. Pheasant tails filling the whole field.—Red jewel motive.—Colored threads running together in a revolving center, the whole similar to a cobweb.—A number of silvery forms in the midst of the field.—Gorgeous vine leaves colored as in Fall, on violet ground.—Arabesques, ornaments, designs changing kaleidoscopically.—Feeling that eyes are open.

1:55. Scintillating gem (size of a nut) deep red in color in center surrounded by greenish dark ground.—Steel veil of gorgeous bluish-violet color.—Visual memory-image of red candy: field turns green.

2:05. Moving rings in different colors.—In center bright light as coming from an electric bulb. Bulb seems to be incomplete. Then brilliant bluish form running through the field; seems to be incomplete in a certain way. In both cases unable to state what is lacking and how the impression of incompleteness arises.—Green sun (diameter about 3 cm.) in bluish dark field; no motion. Then revolving system of splinters of glass; system illuminated from within.

2:15. Homogeneous dark ground with bright patches in left upper field. Suddenly very intense bright light as if produced by an exploding shrapnel (apparently a few cm. distant from the eyes). Almost simultaneously jerky movement of hand to make sure whether blinders are on.

2:25. Darkish figures revolving around a center in a counter-clockwise direction. (Eyes open): streaks of green and violet on the wall. Then a drawing of a head changing into a mushroom (both of natural size). Then a skeleton (natural size) in lateral view turned about 30° to the left. Head and legs are lacking. Try to convince myself that there are only shadows on the wall, but still see the skeleton (as in X-ray).

2:45. (Eyes closed). Soft deep darkness with moving wheels and stars in extremely pleasant colors.—Nuns in silver dresses (about 3 cm. height) quickly disappearing.—Collection of bluish ink-bottles with labels.—Red, brownish and violet threads running together in center.—Autumn leaves turning into mescal buttons.—Different forms emitting intense greenish light.—Forms in different colors; contours often dark.

3:05. (Eyes open): the walls covered with squares (about 2 x 2 cm): shadowy dark contours. The corners of the squares: red jewels (three-dimensional). This design followed by similar mostly more complicated designs in various colors. Sometimes not sure whether the phenomena are localized at the distance of the walls.

3:20. (Eyes closed): on dark ground a red flag on pole waving to the left.—Designs as on rugs.—From now on the opening of the eyes does not neces-

sarily banish the visionary design. The designs seem to be localized on walls, on the floor, etc. The opening of the eyes does not change the magnitude of the forms.[4]

3:35. (Eyes closed): marvellous explosion of scintillating yellowish light mass in center of the field. (Diameter of exploding mass about 4-5 cm.) Very agreeable.

3:45. (Eyes open): impossible to look at the walls without seeing them covered with visionary phenomena. Various designs, then the upper part of the body of an Egyptian woman (two-dimensional, but natural size). (Eyes closed): a jewel design turns into dark background with symmetrically arranged golden sixes which move from the right to the left.

3:55. Strange animal (length perhaps 10 cm.) rapidly turns into arabesques. —Gold rain falling vertically.

4:05. On stationary background rotating jewels revolving around a center. Then, with a certain jerk, absence of all motion.—Regular and irregular, forms in iridescent colors reminding of radiolaria, sea urchins, and shells etc., in symmetrical or asymmetrical arrangement.—Shells illuminated from within radiating in different colors, moving towards the right, turned about 45° towards the right and somewhat towards me. A little piece in every shell is broken out.—Slow majestic movements along differently shaped curves simultaneous with 'mad' movements.—Feeling there is 'motion *per se*.'—Man in greenish velvet (height about 7-8 cm.) jumping into deep chasm.—Strange animal turns into a piece of wood in horizontal position.

4:15. (Eyes opened): visionary phenomena on the walls. Moving the eyes, I notice that the scotoma which I have in my left eye moves before the designs.

From 4:30 p. m. on various experiments were conducted. The visionary phenomena, however, lasted until retiring and could always be seen in the dark room with open eyes. Arabesques, ornaments, various geometric patterns were dominating. On the following day, Oct. 19, after a period of sleep, there was still a great sensitivity to colors, but visions could not be detected with closed or open eyes. On Oct. 20, 10 a. m., I saw one of the designs with closed eyes at the moment when the dentist began drilling one of my biscuspids.

Experiments with Reference to Eidetic Vision

We began with measurements of the size of after-images under conditions similar to those in eidetic experiments (**15**). A gray background (43 x 49 cm., Hering's gray paper 24) was placed at a distance of 50 cm. The head was fixed in a head-rest. Colored squares (Milton Bradley Company) were used as stimuli.

During the experiment the gray background was covered most of the time with ever-changing designs in various forms and colors. Sometimes the paper assumed a cloudy appearance.

[4]Compare E. S. Conklin, (**4**). I do not know whether observations of this kind are in harmony with Conklin's and Jaensch's genetic explanations concerning the approximate constancy of magnitude and deviations from it.

When I tried to touch the slightly pulsating surface my finger seemed to come in contact with it about 20 cm. before the paper. This is a phenomenon surprisingly similar to that experienced by Gelb's patient W (**9**). The paper squares used to produce after-images were also covered with varying forms and colors. Sometimes the phenomena observed on the gray ground showed the complementary color of the square which I was fixating. In most cases, however, the colors were not complementary or even approximately complementary. The normal marginal contrast was frequently observed. The hues of the after-images seemed to be the same as for me under normal conditions. Frequently, the after-images were more deeply saturated. In most cases form and color of the after-images transformed into the visionary phenomena (**16**). A violet after-image which seemed to be identical in size with the original stimulus, rapidly turned into a violet circle, then into stars, etc., which became a part of the design that covered the whole paper. Under these circumstances the determination of the size of after-images was very difficult. In some cases the visions prevented the appearance of after-images altogether. In most instances, however, there was a short period during which an after-image with sharp contours could be seen. Thus its size could be determined (vertical measurements from the outside) before it shifted into the texture of the visionary phenomena.

For illustration we include some records:

TABLE I

Color of stimulus	*Size*	*Distance of exposure*	*Time of exposure*	*Size of negative after-image*	
orange	5 cm	50 cm	10 sec.	measured at 50 cm	5.3 cm
orange	5 cm	50 cm	10 sec.	"	5.2 cm
yellow	5 cm	50 cm	10 sec.	"	5.7 cm
orange	5 cm	60 cm	40 sec.	"	5.3 cm
yellow	20 cm	150 cm	10 sec.	measured at 25 cm	3.6 cm
yellow	20 cm	150 cm	40 sec.	"	3.8 cm

The values obtained are about the same as for me under normal conditions. This implies that: (1) Emmert's law seems to hold (slight deviations); (2) there is the frequently observed enlargement in the isodiastatic constellation (Zeman). The approximate constancy of size in various distances as found in eidetic experiments was never observed under mescal or in the normal state. It was, furthermore, impossible to localize the after-image definitely at a given space, as can be done with the *Anschauungsbilder* of certain persons (**15**). The after-images move with the eyes, while the visionary phenomena seem to be stationary (**16**). It is to be noted that the changes as to color and form in the after-image, which we observed so frequently, are also found in experiments with *Eidetiker*.

In experiments with simultaneous exposure of two 5 cm. squares (either side by side or 5 cm. from each other), two distinct after-images in the complementary color could be seen. A mixture of these squares was not possible. (Sometimes *Anschauungsbilder* can be mixed under the same conditions.) In experiments with monocular exposure (30 sec.), a faint complementary after-image could be seen with the other eye.

I completely lacked the ability of certain *Eidetiker* to see colors spontaneously. The 'intention', the 'will' to see, *e.g.* blue, had apparently no influence. The gray background showed nothing but brilliant jewel and pearl designs in constant motion. Thus a comparison with eidetic experiments in which it was possible to operate with colors which were produced intentionally, cannot be made.

When the paper squares were exposed behind a reading-glass, the removal of the square resulted in seeing an after-image of the size of the stimulus. Simultaneous removal of square and reading-glass did not change this result.

Pictures of animals (cut out of paper: donkey, tiger, etc.)[5] were exposed individually before the gray background for 15-40 sec. After removal I tried to see the picture with open eyes. In no case was I successful; only visionary phenomena covered the ground. The same was found when silhouettes were taken as stimulus material. With the eyes closed, I observed sometimes certain details in the color of the stimulus. They became, however, very rapidly a part of the visionary designs.

The instruction of the experimenter to 'see' a rabbit on the gray ground could not be carried out, though "brilliant red jewels, crabs turning to snakes, green flames" could be seen. The rabbit could not be seen with closed eyes either. Then I tried other animals, but in vain.

It must be said in general that the drug did not create the ability some *Eidetiker* have to see intentionally 'meaningless' (squares, etc.), or 'meaningful' material (pictures, persons, animals), either with open or closed eyes. In all cases visual memory-images of the objects I intended to visualize were present.

In dealing with monocular and binocular estimation of distance under the influence of mescal, I started from an arrangement similar to that in Jaensch's horopter investigations: head in rest; distance of the three black threads from the root of the nose, 1 m.; distance of the threads from each other, 5 cm; occlusion screen 15 cm. before, and gray background 40 cm. behind the threads; the slit of the occlusion screen at the level

[5] See reproduction of pictures and silhouettes in (**15**).

of the eyes. In 15 trials the middle thread was placed at either 95, 100 or 105 cm. (5 times at each distance), the other two threads remaining stationary.

TABLE II

	Monocular			Binocular		
Cm.	*Near*	*Same*	*Far*	*Near*	*Same*	*Far*
Mescal state:						
95	—	4	1	5	—	—
100	—	4	1	—	5	—
105	—	5	—	—	—	5
Normal state:						
95	3	2	—	5	—	—
100	—	5	—	—	5	—
105	—	5	—	—	—	5

Comparing the two series, there is no difference between binocular estimations of distance in the mescal and in the normal state. As to monocular estimation, the result is that, in general, in both states no differences of depth can be perceived. (Compare also (**26**), p. 83.)

It seems, therefore, that mescal could not influence the perception of depth under the above mentioned experimental conditions, in spite of the difficulties of observation: *e.g.* the threads sometimes seemed to move; the gray background was covered with visionary phenomena. It was found, moreover, that in binocular vision 1 or 2 cm. displacements of the right or left thread (98, 99, 101, 102 cm.), the middle and the other thread remaining stationary, were always judged correctly. The threads then were placed at the following distances: middle thread, 95 cm.; right and left thread, 100 cm. The experimenter moved the middle thread slowly backward. It seemed to be the same distance at 99.9 cm. When it was moved from 105 cm. forward, the same distance was reached at 100 cm.

The examination of geometrical-optical illusions (various modifications of Müller-Lyer, Zöllner, Poggendorff, Wundt, Kundt, Hering, etc.) did not reveal any differences between mescal and normal vision so far as the kind of illusion is concerned. With regard to the amount of certain illusions, measurements failed to indicate differences in this respect. Stereograms were normal (**8**). The filling out of the blind spot was performed under various conditions. Hering's "*Gesetz der identischen Sehrichtungen*" was frequently confirmed. Various contrast effects were observed. To illustrate: at 1:30 a. m., Oct. 19, I suddenly noticed a green shadow in peripheral vision: it was the shadow of my red pen holder. The color of positive after-images seen with closed eyes changed into the color of the visionary phenomena. Positive after-images could not be seen with open eyes if colored papers were exposed in day light. It

is well-known that some Eidetiker will get positive *Anschauungsbilder* under these conditions (time of exposure: 1 sec. or less, up to some minutes).

It was possible for me, however, to get positive after-effects of long duration with open eyes if the following arrangement was made: the stimulus material was placed before me in a darkened room and then suddenly illuminated for one second or less. Squares and other geometrical figures, pictures of animals, texts, etc., were used as stimulus material. Certain observations frequently reported by *Eidetiker* were confirmed. Some observations may be recorded. *Stimulus:* a text consisting of 4 lines pasted on cardboard. The words are unknown to me. *After-image:* positive; deep black letters on a paper of extreme brightness; the four lines very distinct; the spatial arrangement of the different words recognizable, but cannot read the text except the words "suffer at all" (correct). The form of the cardboard, and various objects on the table, can be recognized. The brightness on the table is so intense that I have to reassure myself that the light is turned out. *Stimulus:* a text consisting of 6 lines pasted on cardboard. *After-image:* positive; for a moment distinct lines with different words can be seen (unreadable); then the lines turn into rows of black arcades which are alike in form. *Stimulus:* the same. *After-image:* positive; 6 rectangles. *Stimulus:* picture of a tiger: yellow with black stripes and black contour. I do not know what picture is placed before me in the dark. *After-image:* my right hand holding the picture of the tiger; the texture of my coat, the shirt, folds on my thumb quite clear; the picture of the tiger itself in brilliant yellow with deep black contour and stripes. To the left my left lower arm, the hand at the key of the lamp, the lamp itself are seen. As to hue, form, voluminousness, the objects do not seem to differ very much from real objects. Finally the yellow of the tiger turns into a bluish color, the stripes and contour remain black. (During the observation of the after-image I held my hands and lower arms under the table. Probably owing to certain changes in the kinesthetic field, they seemed to be detached from me in a certain way. Thus my 'real' limbs and the limbs in the after-image seemed to be equally 'real' or 'unreal;' a confusing effect). *Stimulus:* contours of circles (colored paper, 2-5 mm. wide), squares, triangles, etc.; certain parts of the contour covered up by white paper. *After-image:* in all cases the form of the original stimulus is retained; no completion of form takes place. (Compare (**26**), p. 149.) *Stimulus* (monocular exposure): square or picture or candle. *After-image:* seen by the eye to which the material was exposed; opening of the other eye produced blinding effect, and increase in darkness while the stimulated eye still sees a distinct and brilliant after-image;

closing of the stimulated eye produced complete darkness. *Stimulus:* the same; the one eye exposed to the material, the other eye open, but covered by a screen. *After-image:* seen by the eye previously stimulated; no blinding effect in the other eye; closing of the stimulated eye produced complete darkness; no binocular effect of monocular exposure.

It is to be understood that the various after-sensations arising after short illumination in a dark room were soon followed by the appearance of visionary phenomena. They could be seen both with open and with closed eyes. The sudden illumination banished the phenomena in most cases completely for the time of the duration of the after-image.

The Marburg School has found that the application of disturbing stimuli (whistling, various noises, etc.) influence *Anschauungsbilder* in a definite way. Compare (3). The effect of various auditory, olfactory and cutaneous stimuli on mescal visions was tried. After the application of auditory stimuli I did not observe any change in the visions. It did not make any difference whether or not I expected the sound or noise. Upon application of olfactory or cutaneous stimuli the form of the visionary design as well as the saturation of the colors changed suddenly in some cases.[6] In other cases there was no effect at all.

Changes

In different Sensory Fields. With regard to color vision, there was a general increase in brightness; most colors seemed to be more deeply saturated. These changes in brightness and saturation seemed especially noticeable in peripheral vision. Again and again, the 'optical attention' was attracted by objects in the peripheral field; *e.g.* a 'flame' in the right lower field (twilight) turned out to be a piece of paper in the waste-basket; the hitherto unnoticed reflection, in one of a friend's front teeth, of a gold inlay in the next tooth was observed. In foveal vision objects seemed to become loose and cloudy. It was strenuous work to fixate objects for a longer time. Sometimes they seemed to lose their solidity. Colors often showed those characteristics usually ascribed to *Flächenfarben* (film colors). Movements of objects were frequently observed. Looking at the radiator in the room, its divisions seemed to move rhythmically. Standing near a corner of the room, the walls, like yellowish clouds, seemed to move towards me. Mach's spiral appeared to be a black steel spring, lying on brilliant white chalk, performing slow contractions. A package of cigarettes on the table moved with a jerk. A black circle pasted on white cardboard: I tried to dip my finger into a deep hole. (Foveal vision! compare the

[6] See also (7).

above mentioned observation with the gray paper and Bühler, (2) p. 57.) During the evening, the room being illuminated by electric light, the white paper on which I was writing was yellow; the walls, ordinarily of a dull grayish-yellow, showed a deeply saturated yellow.

With regard to auditory stimuli, even noises caused by writing, swallowing, etc., seemed to be loud. Tones of my violin had a greater voluminousness than usual. Trying to strum a melody sung by the Indians at their peyote festivals I noticed that I could not keep time. With regard to organic sensations, I seemed to be 'beyond all desires.' I did not feel hungry or thirsty. From the evening of Oct. 18 until noon of Oct. 20, I ate only some slices of bread and some bananas. Even at 2:00 a. m., Oct. 19, I was wide awake; there was hardly any feeling of fatigue throughout the experiment, although there were times when I was extremely drowsy and disinclined to move. In general, frequently repeated gymnastic exercises seemed to be amusing rather than fatiguing. Walking was performed with remarkable ease.

In 'Gegenstands- und Ichbewusstsein.' Some alterations besides the sensory changes are to be mentioned. My state may be characterized, on the one hand, as one of enchanted contentment, in which I was happily observing the visionary world; on the other hand as restless and wide awake, being eager to taste thoroughly the unique experience. There was no conscious intention, however, to be forcibly attentive to the phenomena or to slight sensory impressions. A certain unwillingness to be intellectually active could be noticed throughout the whole day. At the same time there was a dislike to formulate my experience verbally for the sake of an experiment. Even the idea of being subjected to experimentation was unpleasant. To the experimenter I seemed to be very distractible. Introspectively there seemed to be a certain disinclination, rather than inability, to focus my attention on anything for a longer time. There were no difficulties in understanding some pages of a psychological paper, but the whole process was rather disgusting.

The distortion of time frequently recorded by other experimenters was also experienced; but in this experiment it was apparently not so strong as in other cases. After an unpleasant period of 'concentration' it was possible to make fairly accurate time judgments (*Cf.* 7). According to some investigators an enormous over-estimation of time is very characteristic. In my state it seemed most characteristic that the inadequacy of assigning durations was felt so keenly. A question of the experimenter concerning time was considered rather ridiculous. It seemed to me incommensurate to speak about the experienced abundance of phenomena in terms of minutes and hours. I re-

call that in the past a similar attitude with regard to time has arisen in situations with high emotional pressure, *e.g.* in periods involving danger to life.

One of the experiences (a most surprising one) of this experiment was that owing to certain optical changes in the external objects, movements of external objects, film colors instead of surface colors, etc., visions and external objects became phenomenologically similar. Besides this unification in perceptual respect there are other changes of the *'Gegenstandsbewusstsein'* and *'Ichbewusstsein.'* My body and its organs seemed to be most of the time non-existent or detached from me as a perfectly functioning machine. While speaking I seemed to listen to a speech apparatus. Getting up from my arm chair to move around, I seemed to walk on clouds. In a moment of exhilaration I was inclined to kick through the airy walls of my room with my foot, knowing at the same time that I was in a solid stone building.

In general, the line of demarcation drawn between 'object' and 'subject' in normal state seemed to be changed. The body, the ego, became 'objective' in a certain way,[7] and the objects became 'subjective.' They became subjective not only in the sense that they behaved as visionary phenomena, but also in the sense that they gained certain affective qualities. A movement of the hand of the experimenter, a noise, a turning of the chair, seemed to be harmful and dangerous. Critically, I realized that the movement of a hand at 2 m. distance could not be injurious; but nevertheless the harmful influence of such a movement was felt directly. Somehow, the physical medium of the air, the difference between the bodies did not exist. I frequently felt that the experimenter meant me ill and I wished he would abstain from thoughts which could do me harm.

There is no doubt that these changes in *'Gegenstands- und Ichbewusstsein'* are comparable to those observed in schizophrenia. For the psychology of schizophrenia as developed by Storch, Schilder, and Kronfeld, etc., as well as for genetic psychology (compare Werner), mescal experiments might play a part in throwing light upon the genesis and pathology of the subject-object relation. And mescal seems especially recommendable, since it does not destroy the critical attitude of the observer.

Suggested Experiments

In closing, some further mescal experiments may be suggested. It seems desirable to test the effect of mescalin on: (1) a congenitally blind person; (2) a totally or partially color-

[7]Compare one of H. Ellis' subjects (7): "It was as if I had unexpectedly attained an objective knowledge of my own personality."

blind person; (3) various *Eidetiker*, especially on persons of the T-type; (4) persons of the T-type who lost the eidetic disposition after ingestion of calcium lacticum; (5) persons of Kretschmer's body-form types, to investigate systematically the changes in '*Gegenstands- und Ichbewusstsein.*'

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GESTALT PSYCHOLOGY AND MOTOR PSYCHOLOGY[1]

By MARGARET F. WASHBURN, Vassar College

Most American psychologists feel, I think, a cordial gratitude towards *Gestalt* psychology, particularly to the two missionaries, Professors Koffka and Köhler, who have expounded it so delightfully in this country, for the keen introspection and ingenious experiments by which they have revealed to us certain truths about our conscious experience, and reminded us of many more which nearly all of us have long acknowledged, but have perhaps neglected always to emphasize in our writings. We have not, indeed, quite recognized ourselves in the portrait they have drawn of that person whom they call the associational psychologist. We cannot admit that we regard conscious experiences as bundles of sensations, we who in our youth learned thoroughly from William James the law that all things fuse that can fuse and nothing separates except what must. But we do confess that we have sometimes seemed to overlook these laws, and we delight in having them pointed out to the extreme behaviorists, who have wholly ignored them.

My own difference with the *Gestalt* psychologists lies in the following point. I am willing to grant all that they urge against an atomistic conception of consciousness; I recognize the fact that the total conscious state is in no sense a mosaic, but a continuous blended whole. What I miss in all their discussions is any mention of the factor that seems to me to play the leading rôle in the phenomena they so vividly describe: namely, the nature of the motor response. It is this factor which appears to me to form the very essence of that 'thing character' which constitutes a configuration.

Take as an illustration the familiar pair of illusions shown in the accompanying figures. In Fig. 1, circle A looks larger than circle B, although they are equal by measurement. In Fig. 2, the small circle in C looks larger than the large circle in D, although they also are equal. A partial explanation of these illusions consists in saying that A looks larger than B because A is compared with the small circles around it, while B is compared with the large circles around it: the principle of contrast governs. The small circle in C (Fig. 2), on the other hand, is assimilated to the large circle around it and looks larger than the large circle in D, which is assimilated to the small circle within it. The configurationist might say that the patterns in which A and

[1]Read at the Ithaca meeting of the American Psychological Association, Dec. 28, 1925.

B are found are such as to give emphasis to the circles as separate from one another, so that they are compared with one another and the principle of contrast rules; while in the case of C and D the principle of assimilation holds. But one looks in vain for a deeper-lying principle to explain why one figure should produce emphasis and another assimilation. What, exactly, is

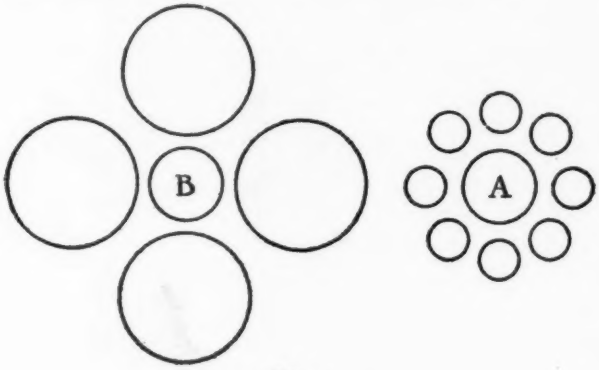

FIG. 1

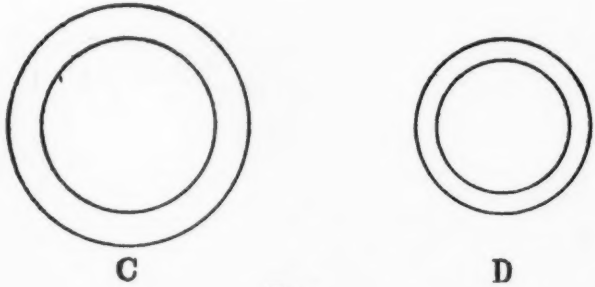

FIG. 2

the nature of emphasis? We never learn from the configurationist. If, however, we introduce the influence of the motor response suggested by the figures, the matter becomes clearer. In Fig. 1 the circles are so placed that *it would be easy to pick them up separately*. They are therefore perceived as separate things and compared with one another. On the other hand, in C and D (Fig. 2), the concentric circles are so placed that one would naturally pick them up together. They look, in fact,

much like a plate. Hence it is that they are perceived as a single thing, and not being compared with each other, the consciousness of their difference is lessened.

What is the real nature of those patterns which the configurationists call good or natural configurations? They are patterns whose elements are so arranged as to suggest reaction by a single movement to the whole group. Why is it that one portion of a field is perceived as pattern and the remaining

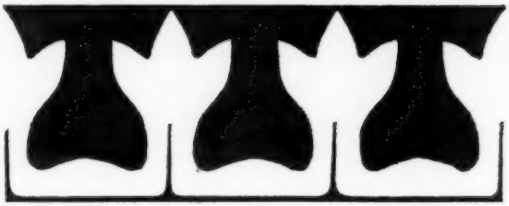

FIG. 3

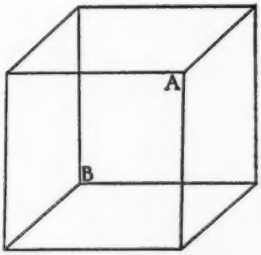

FIG. 4

portions as background? Köhler says that a small figure on a large ground receives emphasis and is perceived as figure rather than ground because the electric currents in that portion of the retina corresponding to the small figure are more concentrated than those in the surrounding portion. This may be true, but a further explanation lies in the fact that the small figure looks easier to pick up from the background than the background does from the figure, on the basis of our lifelong experience with picking up things. Where, as in the case of the pattern in Fig. 3, the suggestions of movement are equally strong in the two patterns, the white looking as easy to pick up from the black as the black does from the white, either white or black may receive the emphasis. Köhler would say that the concentration

of currents is equal in the two figures. Now in such a figure there actually occurs an alternation of emphasis, a shift from one to the other, the black and the white alternating as pattern and ground respectively. What determines this shifting? No obvious reason appears for any change in the concentration of currents. But on the motor hypothesis, anything may cause the shift which suggests a change in the motor response from picking up black to picking up white or vice versa. The words 'white' or 'black,' or a change of fixation point due to fatigue, may do it. In the ambiguous cube, Fig. 4, whether a cube resting on the ground or one suspended in air is perceived may depend on whether one fixates the point A or the point B; the point fixated seems to be nearer, that is, suggests a shorter reaching movement, because, other things being equal, in looking at an object we generally look at its nearest point, which of course carries with it that surface of the cube to which it belongs. Further, if a record is kept of the duration of the phases of the cube, we usually find that it is seen longer as a cube resting on the ground than as a hanging cube; this may be explained by the fact that we have a stronger tendency, due to habit, towards sitting down on cubes than towards looking up at them.

These illustrations indicate how the phenomena of ground and figure, of assimilation and emphasis, can be explained by the motor responses suggested, usually from previous experience. So far as I can learn, they are not explained at all on the configuration psychology; they are merely described.

In the second place, a motor psychology can offer an explanation for those relational processes which play so large a part both in configurationism and in actual conscious experience. About ten years ago, in my book, *Movement and Mental Imagery*, I tried to sketch such an explanation, and it still seems to me a serviceable one. It is briefly as follows. The conscious effect of a stimulus pattern is simple when the pattern is reacted to as a whole, complex when details are reacted to. Now the reactions themselves, of whatever nature, give rise to kinaesthetic excitations, and this whether they are movements fully performed or incipient, tentative movements only. These kinaesthetic excitations are a regular accompaniment of such excitations from outside stimuli as are reacted to at all; they influence all conscious processes. Whether they themselves are directly represented in consciousness depends on whether they in their turn are reacted to. When the mass of kinaesthetic excitation is reacted to as a whole, we have in consciousness a simple feeling of relation; when parts of the mass of kinaesthetic processes are reached to separately and thus localized in the body, we experience a complex of kinaesthetic sensations. Often they are

not reacted to at all; our reaction habits are formed under the influence of practical needs, and we can get on much of the time very comfortably without being aware of relations as such, and all of the time without analyzing our feelings of relation into kinaesthetic sensation complexes: but whether we react to these kinaesthetic processes or not, they and the movements underlying them profoundly modify our experience. The simple relational feeling of *but*, we may suppose, arises when incompatible movements tend to be aroused; the simple relational feeling of difference when there is a sudden shift of motor excitation; grades of intensity may depend on the felt degree of diffusion of the motor response; feelings of duration, as Wundt has suggested, on strains and relaxations, and so on. If relational experiences have this kinaesthetic origin, it is easy to conceive them as occasioned not by absolute stimuli but by changes in the relation of stimuli, that is, changes in the diffusion, location, duration, combinations, and so forth, of the movements, full or tentative, performed by the organism. There is nothing puzzling, for example, about the fact that while intensity A may be indistinguishable from intensity B and intensity B from intensity C, intensities A and C are distinguishable from each other.

The most important evidence for this kinaesthetic theory of the nature of relational processes seems to me to be language. It would be only rarely and accidentally that the same verbal response would be attached to two different experiences if they had no common factor. If then, we use the same words, such as 'more' and 'less,' 'like' and 'different,' to designate quantitative change in any kind of conscious content whatsoever; 'longer' and 'shorter' of both temporal and spatial extents; 'soft' of touches and sounds, and 'motion' of both a tactual and a visual experience, this is most probably because of common factors accompanying the various sensory experiences. What more plausible suggestion as to the physiological origin of these common factors than that they come from our own reactions, muscular and organic, to the external situations which we encounter; from that source of experience which we carry constantly about with us, our own bodies?

WHAT IS EMPIRICAL PSYCHOLOGY?

By LEONARD CARMICHAEL, Princeton University

The phrase *empirical psychology* is used in at least three major senses in current technical psychological discussion. It is the purpose of the present paper to consider the following questions in regard to this triple usage: (I) What is the definition of each of the three meanings of the phrase? (II) Are these three meanings in any manner related? (III) Is it expedient to continue using the same phrase to denote three meanings?

I. DEFINITION OF THE DIFFERENT MEANINGS

In the first of the three senses to be considered here, the word *empirical* may be taken as roughly synonymous with the word experimental.[1] This usage enjoys the sanction of historicity. Among the sects of ancient physicians the *Empirics* drew their procedures entirely from practice and trial: they tried to exclude all philosophic theory. The *Empirics* opposed both the *Methodic* and the *Dogmatic* schools.[2] Hence it has come to be that one is characterized as *empirical* "who either in medicine or in other branches of science relies solely upon observation and experiment."[3] In this sense the term is antithetical to such words as *rational* and *inferential*. This antithesis is one of method. Indeed, *empirical*, in this first sense, is strictly a methodological conception. Before the middle of the eighteenth century, Christian Wolff dichotomized psychology upon the basis of this anthithesis. He distinguished between a psychology which is in method *empirical* and one which is in method *rational*.[4] In spite of Wolff's failure to live up to this program of separation there are still psychologists who attempt to divide the discipline

[1]Strictly speaking, *empirical*, in this sense, is a broader term than *experimental* which rightly designates a kind of experience in which the fact observed has been isolated, to some extent, by intentional pre-arrangement of conditions. Cf. William Hamilton, *Lectures on Metaphysics*, 1859, I, 55. Thus the true experimental method is seen to be always an empirical method. Similarly any empirical observation is or was subject to experimentation. The conception of the *Gedankenexperiment* of Ernst Mach is, of course, no exception to this generalization for this method is only by analogy experimental; it is typically rational. Cf. *Erkenntnis und Irrtum*, 1906, 183-200, and for distinctions between this rational method and true experimentation E. Rignano, *The Psychology of Reasoning*, 1923, Chap. IV.

[2]Cf. W. Whewell, *History of the Inductive Sciences*, 1847, III, 425 f.

[3]*New English Dictionary*, Vol. III, Part I, 128.

[4]Christiano Wolfio, *Psychologia Empirica Methodo Scientifica Pertractata qua ea, quae De Anima Humana Indubia Experientiae Fide Constant, Contentur, etc.*, 1732, par. 1-10. Cf. also, *idem*, *Psychologia Rationalis, etc.*, 1734.

in this same fashion. For example, Maher says: "The discussion of the former questions—the inquiry into the character of the various mental states and operations is called by different writers *Phenomenal, Empirical,* or *Experimental Psychology;* whilst investigation into the nature of mind itself is styled *Rational Psychology* . . . The term *Phenomenal* is applied to the first part of Psychology, because it investigates the various *phenomena* of the mind, the facts of consciousness. It is called *Empirical* or *Experimental* because we have immediate experience of these facts: we can study them in immediate observation. The second part of our subject is marked by the epithet *Rational,* because the truths which are there enunciated are reached not by direct experience, but by reasoning from conclusions established in the earlier part.[5]

The methodological limitations, therefore, of empirical psychology considered in this first sense are such that it may establish only generalizations that will make each law a uniformity whether of succession or co-existence and which holds true in all instances within our limits of observation, but is not of a nature to afford any assurance that it would hold beyond those limits.[6]

In this first sense of the term, then, the adjective *empirical* may be thought of as characterizing a psychology which is based upon experimentally discovered data.[7]

(2) In the second of the three senses to be considered here the word *empirical* may be taken as roughly synonymous with the phrase, *acquired during individual development.* While current in much psychological writing this meaning is essentially epistemological. In this sense the term is antithetical to nativistic.[8] This meaning of *empirical* is frequently used to-day. Thus, Warren says, "This application of the evolution theory to psychology gave the *a priori* school a nominal victory, since its contention was literally substantiated; but empirical psychology came off with real honors, since its protest against 'innate

[5]M. Maher, *Psychology: Empirical and Rational,* 1900, 5.

[6]J. S. Mill, *System of Logic,* Book VI, Chap. V, par. I, 1887.

[7]By a curious chain of circumstances (which may well be a parable for any discipline which attempts to be *purely* empirical), this honorable word has come to have, in certain circles, an unfavorable connotation. In medicine the term is often used to describe a charlatan or a quack. This same usage has been extended to other fields. The unfavorable meaning of the word arose from the fact that the ancient Empiric school of medicine degenerated. Its errors, which may well have been due to a failure to correlate observations, came (first in the influential works of Galen) to be synonymous with ignorance and false pretension. Cf. William Hamilton, *loc. cit.*

[8]Cf. J. M. Baldwin, *Dictionary of Philosophy and Psychology,* 1901, I, 321; also R. Müller-Freienfels, *Eislers Handwörterbuch der Philosophie,* 1922, 522 f.

forms' was validated in the wider field of race history."[9] Klemm uses the word in a similar sense, "The typical supporter of empiricism in the theory of spatial perception in Germany is Helmholtz. According to his empirical view our sensations are signs for our consciousness, the significance of which our reason has to learn."[10] Or, as Koffka says, "Even to-day no agreement has been reached between the rival theories of *empiricism* and *nativism*, the first of which emphasizes the influence of the environment, and the second the influences of heredity."[11]

In this second sense, then, the term *empirical* may be thought of as descriptive of a psychology which holds that behavior and experience are wholly developed in the ontogeny of the individual.

(3) The third usage of the phrase *empirical psychology* is not so easily characterized. It has not yet found its way into the lexicons. Its sanction is derived largely from its iterated use by Professor E. B. Titchener. The special sense in which this scholar uses the words *empirical psychology* therefore may best be given in quotation from his own writings.

In an article entitled *Brentano and Wundt: Empirical and Experimental Psychology*, this author shows the antithesis which he believes to exist between the two tendencies presented in his title. In the first place the empirical psychology of Brentano is characterized as "essentially a matter of argument;" while the experimental psychology of Wundt is "essentially a matter of description."[12] Again, we are told: "For the 'empirical' psychologist means to take mind as he finds it; and like the rest of the world,'who are not psychologists, he finds it in use; he finds it actively at work in man's intercourse with nature and with his fellow-men, as well as in his discourse with himself."[13] In a later article, in considering the hindrances to the development of an experimental psychology, as he conceives it, Titchener further writes:

"The second unfavorable influence which I have to note is of far greater importance—partly because of its antiquity and perennial vitality, partly because of its seeming innocence and the guise of kinship with experimental psychology which it often assumes, so that it works insidiously from within rather

[9]H. C. Warren, *A History of the Association Psychology*, 1921, 119.

[10]O. Klemm, *History of Psychology*, tr. by E. C. Wilm and R. Pintner, 1914, 335.

[11]K. Koffka, *Growth of Mind*, tr. by R. M. Ogden, 1924, 1.

[12]E. B. Titchener, Brentano and Wundt: empirical and experimental psychology, this JOURNAL, 32, 1921, 111.

[13]*Ibid*, 119.

than openly from without. It is the influence of empirical psychology, a mode of psychology that goes back to Aristotle and Thomas Aquinas, and that forms the staple contents of most psychologies down to and including our twentieth-century textbooks. The empirical psychologist professes to take mind as he finds it: I use the term 'mind' without prejudice, for the subject-matter of empirical psychology; in point of fact the definition of that subject-matter has varied greatly: always, however, the empirical psychologist professes to take mind as he finds it. . . . This subject-matter, then, mind in use, the empirical psychologist seeks to reduce to order and arrangement; and empirical psychology thus becomes a rationalization of practice, a 'description and explanation' of mental uses." [14]

These excerpts suggest the meaning which Titchener has newly attached to the term *Empirical Psychology*. For a complete picture of his view the two articles cited must, of course, be read in their entirety.

If, in summary, a short characterization of the meaning of this phrase, as used by Titchener, be essayed, it may be suggested that an *empirical psychology*, so considered, is a discipline concerned with the presentation of a consistent and reasoned account of the mind in use. Such a psychology, typically, respects the authority of past thought more than the data of present experiment. *Empirical psychology* is therefore, in this sense, antithetical to an hypothetical psychology that is purely descriptive.

II. Relation Between the Different Meanings

Are these three general meanings of the phrase *empirical psychology* related in any manner? Between the first and second meanings considered above there may be some kinship. The hypothesis of nativism, historically, seems to have been, in so far as psychology is concerned, for the most part, although by no means exclusively, supported by those who use some such method as intuition rather than experiment for the discovery of the facts which they consider.[15] To the present writer it seems more than a mere coincidence that the psychology of John Stuart Mill was empirical, in spirit at least, both in the sense

[14] *Idem*, Experimental psychology: a retrospect, this Journal, 36, 1925 315 ff. Cf. also K. M. Dallenbach, Review of Woodworth's *Psychology*, *ibid.*, 33, 1922, 434 ff.

[15] Of course this is by no means universally true. In claim, the post-evolutionary psychology of instinct is empirical in method.

that it opposed 'innate principles' and that it depends, at least theoretically, upon scientific experiment for its data.[16]

As we have shown, the first conception of *empirical* is methodological; the second, epistemological. It may be urged that a proper application of the empirical method to the data of development will show that all mental life is empirically (in the second sense) acquired; but this is a separate question which cannot be considered here.[17]

Between either the first or the second meaning of the term *empirical* and the third sense in which the adjective is now used, there exists even less relationship. There is in fact between two of these a direct opposition. Titchener himself says, "For all experimental psychology is in the broad sense empirical, and a psychology which is in the narrow sense empirical may still have recourse to experiment."[18] But "in the narrow sense" an *empirical psychology*, in this very *Brentano and Wundt* article, is contrasted with an *experimental psychology*.[19] Hence we find an antithesis between the "broad" and the "narrow" meanings of the same phrase! A double usage of this sort is calculated to confuse the modern reader.

Certainly, little or no relationship between this novel third meaning of the phrase *empirical psychology* and the second, or epistemological, use of the word as *non-nativistic*, can be demonstrated. If *empirical*, in the third sense, be taken as synonymous with *metaphysical* or merely *speculative*—words which in fact do not do justice to the important conception of *empirical psychology* as set forward by Titchener—it might be urged in the light of history that the empirical psychologist is characteristically a nativist. Such a contention, however, would be subject to many exceptions. As suggested above in the considera-

[16]Cf. J. S. Mill, *Autobiography*, 1924, 158, and *idem*, *System of Logic*, 1887, Books V and VI. Wundt may have been the first to use the phrase *experimental psychology*. But, in regard to the origin of this conception, Titchener has said: "Whence now did Wundt derive his idea of an experimental psychology? . . . No one can read the introduction to the *Beiträge* without being reminded of the sixth book of John Mill's *Logic*; and no one, I think, who after such reminder compares the two compositions can doubt that Mill, for whom psychology is explicitly a science of observation and experiment, gave the cue both for Wundt's emphasis on improvement in method and for the concrete means to improvement, statistics and experiment, which Wundt propounds" (Wilhelm Wundt, this JOURNAL, 32, 1921, 164f.).

[17]I have previously considered certain phases of this problem in two papers: Heredity and environment: are they antithetical? *J. Abn. & Soc. Psychol.*, 20, 1925, 245-260; and Development of behavior in vertebrates experimentally removed from the influence of external stimulation, *Psychol. Rev.*, 33, 1926, 51-58.

[18]Titchener, Brentano and Wundt, *op. cit.*, 111.

[19]*Vide supra.*

tion of Mill's system, it may well be that the correlation between methodological empiricism and a non-nativistic view of mind is more than coincidental; but it is a relationship of such an involved sort that it surely cannot be used to define the word *empirical*.

It may be said with assurance, then, that between the three uses of the phrase *empirical psychology* no logical relationship exists which may serve to make homogeneous their disparate meanings.

III. Is it Expedient to Continue the Present Triple Use?

In light of the fact that the three usages of the single phrase *empirical psychology* are not only incongruous, but in certain instances positively antithetical, is it expedient to continue the present triple use? It is the contention of the present paper that to do so is confusing and therefore inadvisable. The first meaning we have seen is historical: broadly speaking, the *empirical* method *is* the *experimental* method. In this sense the term is well established and may continue to be used. Secondly, the term, *empirical*, as characterizing a psychology of individual acquirement is legitimate enough.[20] To the present writer, however, it would seem on the whole better to substitute the term *ontogenetic* psychology in place of the epistemological use of the phrase *empirical psychology*.

In regard to the third use of the term *empirical psychology*, we should at once agree that a writer may coin a new phrase, or, as in the present instance, add a new meaning to an old phrase if he so desires. In the present case, the significant and critical conception which Titchener has built up around the phrase *empirical psychology* is novel and illuminating. Nevertheless, it is unfortunate that the phrase chosen is ambiguous. To understand the words *empirical psychology* in this new sense, demands a radical acceptance, in a curiously literal fashion, of the contextual theory of verbal meaning. This fact makes it all the more difficult to suggest a new word that will 'carry' the meaning which now 'accrues' to the adjective *empirical* in Titchener's writings. The old term *rational*, however, seems to fit the case rather precisely.

In the way of this substitution, it is true, there are obvious difficulties. For example, its adoption would entail the assertion that Brentano's so-called *empirical system*, in so far as it is not really based upon the results of the empirical method, is a

[20]It will, of course, be remembered that fundamentally, and in derivation, *experimental* and *experiential* are not unrelated. Cf. A. Bain, The empiricist position, *Mind*, 14, 1889, 369-392.

misnomer.[21] But after all is this not the case in Brentano's psychology? Does not a clear use of terms always make conceptions themselves more succinct?

In conclusion then it seems that less ambiguity would result if *empirical*, in the first sense considered above, be still used as broadly synonymous with experimental, that *empirical* in the second sense be supplanted by *ontogenetic*, and that *empirical* in the third sense, be supplanted by *rational*.[22]

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[21]Cf. F. Brentano, *Psychologie vom empirischen Standpunkte*, 1874. It is interesting to note that Titchener has been characterized as an empirical psychologist. Cf. Vernon Lee, Introduction to Semon's *Mnemic Psychology*, 1923, 34.

[22]It may be further asked, if the present terminology is adopted, whether, in practice, an empirical psychology can be completely independent of a rational psychology? The present writer is inclined to believe that a pure empirical psychology cannot be developed. Premises may be concealed: metaphysics may be implicit: but in psychology, as in physics, certain fundamental assumptions must be made if the scientific data are to present a seemly and systematic order. The empirical method, purely applied, might well result in a 'scrap-book' of irrelevant items. In this day of conflicting 'scientific' systems of psychology, which agree in little save that they are 'non-metaphysical,' certain words of Professor Dewey, written some years ago, are still important. He says: "No writer can create nor recreate his material, and it is quite likely that the philosophic implications embedded in the very heart of psychology are not got rid of when they are kept out of sight. Some opinion regarding the nature of mind and its relations to reality will show itself on almost every page and the fact that this opinion is introduced without the conscious intention of the writer may serve to confuse both the author and the reader" (*Psychology*, 1886, p. iv.).

THE EFFECT OF VARYING PERIODS OF ADAPTATION ON THE FLIGHT OF COLORS*

By AUDREY M. SHUEY, Northern Illinois State Teachers' College

INTRODUCTION

In a previous article the writer has described an experiment on the flight of colors in the after-image of a bright light.[1] The purpose of that experiment was to investigate the relation between the after-image and the varying intensities and durations of stimulation which gave rise to it. As a fixed period of five minutes' adaptation to darkness conditioned every observation, the possible effect of different durations of adaptation on the image was not investigated. The present paper describes further experiments on the after-images following various degrees of adaptation to darkness.

Apparatus. The observations in this experiment were conducted in a dark room with apparatus identical, with one exception,[2] to that of the previous experiment.[3] In place of one of the four 'day-light' mazda bulbs, rated at 25, 50, 75, and 100 w., a stimulus light, rated at 75 w., was used throughout the investigation. Furthermore, a constant duration of 40 sec. exposure to this stimulus conditioned every observation.[4]

Subjects. All of the 28 *S*s were undergraduate students who had had at least one course in psychological training. Twelve *S*s—An, Ed, Gr, Ha, Lu, Ne, Oa, Pe, Ra, Su, To, and Wi—reported 37 times each in the first experiment which continued 4½ months. Eleven *S*s—Ba, Co, Fe, Ho, Kr, Lh, Pr, Ro, Se, St, and Wh—each reported for 5 observations during a six weeks' summer session. The 5 *S*s of the third group—Ab, Fr, Ri, Sc, and Sl—each served 100 times during a period of 12 weeks. Altogether 695 observations were completed in the experiment which lasted 9 months.[5] With the exception of 7 subjects—Ha, Pe, Ra, Kr, Pr, St, and Fr—whose eyes were corrected by lenses, all the *S*s had apparently normal vision.

The experiments were performed at regular periods between 8:00 A. M. and 4:15 P. M.; consequently the *S*s always entered the dark room from the light. No check was made upon the daylight which varied in intensity from day to day. The varying intensities of the pre-darkness period probably affected the after-images following zero and short adaptations to darkness, as the report of one *S*, who had been looking out of a window before entering the dark room, clearly shows. This *S* reported after no adaptation a long rectangular blue background around an imaged cross.

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*Communicated by C. A. Ruckmick.

[1] A. M. Shuey, The flight of colors, this JOURNAL, 35, 1924, 559-82.

[2] A 2.8 v. flashlight battery instead of a transformer provided current for the 'ready' signal.

[3] For description of apparatus used cf. *op. cit.*, 561-62.

[4] The fact that the 75 w. bulbs with 40 sec. exposure are relatively moderate in intensity and duration made them desirable as uniform stimuli in this problem.

[5] The writer acted as *E* throughout the experiment.

Experimental Procedure

A preliminary experiment conducted to give *Ss* practice in describing their images preceded the principal observations. In this series *E* tested 0, ½, 1, 2, 3, 4, 5, 6, and 7 min. adaptations to darkness. As a result of this procedure, 0, 3, 5, and 7 min. were used as the various lengths of *Ss'* adaptation before they were subjected to the light stimulus. The latter part of the experiment included longer periods of adjustment to darkness, *i.e.* 15, 30, 45, and 60 min. During the period of adaptation[6] before each observation *E* read the following instructions to *S*:[7]

"At a given ready signal followed by the flash of light, you will steadily fixate the black spot in the center of the Greek cross and continue to do so during exposure to the stimulus. As soon as the cross is illuminated report all visual changes noticed. After exposure, describe as fully as possible all visual effects and changes, including quality and form. Proceed from the center of the image outward. Signal as soon as you experience any image."

After she gave the directions *E* said 'Ready,' flashed the signal, said 'Now,' and switched on the stimulus light and started the stop-watch. At the end of 40 sec. *E* turned off the current and reset the stop-watch immediately to time the after-image effects and the length of the total image. *E* noted in a column at the left of the report the second at which each statement in the observation was given.[8] After every experiment *E* questioned *S* concerning his images, the rapid alteration of which occasionally prevented his giving all their characteristics as they appeared. *E* was in doubt as to whether the *Ss* were all experiencing the same qualities as their descriptions indicated. Consequently she asked *S* to compare the imaged colors[9] in their different stages with the Milton Bradley rainbow, tonal, and tinted drawing papers. *S* likewise signified the various changes in the size and shape of the image by reproducing them on paper.

Results

Course of the Image: (*a*) *Rise of Image.* As the conditions for both series were identical, results of the first observations following adaptations of 0, 1, 3, 5, and 7 min. will be examined in this paper with those following 0, 15, 30, 45, and 60 min.[10] These results were separated according to the characteristics of the *Ss'* images which are indicated by (1) rise and subsequent dura-

[6]When *0* adaptation preceded an observation *E* read the in.tructions to *S* before he entered the dark room.

[7]*Op. cit.*, 562-63.

[8]*Ibid.*, 563.

[9]The following symbols were used in recording the colors: *R*–red, *B*–blue, *Y*–yellow, *Li*–light, *L*–lavender, *Bn*–brown, *Br*–bright, *Pk*–pink, *P*–purple, *G*–green, *g*–gray, *Bk*–black.

[10]The italic numerals *0, 1, 3, 5, 7, 15, 30, 45,* and *60* are used to indicate respectively 0, 1, 3, 5, 7, 15, 30, 45, and 60 min. of adaptation.

tion of image; (2) hue, tint, and saturation of image; (3) fluctuations in image; (4) size and shape of image; (5) position of image; and (6) rivalry and movement of color in image. Table I following shows the relation between the length of adaptation and the moment of appearance of the after-image.

TABLE I

Showing length of adaptation and moment of appearance of after-image (Sixty-six observations were obtained at every length of adaptation).

Length of Adaptation	First Appearance of the Image (Seconds)								
	1	2	3	4–7	8–10	11–15	16–20	21–30	31–40
0	**17**	6	12	**17**	10	4			
1	15	6	**23**	11	6	3	2		
3	14	**16**	14	**16**	4	2	1		
5	15	12	**25**	8	2	3	1		
7	13	9	**21**	17	5	1			
15	7	18	13	**21**	2	2		3	
30	9	5	12	**28**	9			2	1
45	9	13	11	**16**	10	7			
60	5	16	4	**20**	14	5	2		

As is indicated in the table the greatest number of images following *0* adaptation and the least after *60* appeared within 1 sec. after removal of the stimulus. As adaptation was increased, therefore, the number of images that appeared immediately decreased. More than one-half of the images following brief adaptations, *i.e. 0, 1, 3, 5, 7,* and *15,* appeared within 3 sec. upon removal of the stimulus while following *30, 45,* and *60,* the greater number came after 4 sec. Practically all of the images following every adaptation, however, appeared within 10 sec. after stimulation had ceased.

There were marked individual differences in the images as is indicated by the fact that Lu's, To's, and Pe's images often first appeared later than 4 sec. after every adaptation. 48 of the 60 images that came after 7 sec. from the removal of the stimulus were images of 6 *S*s—of Lu, To, Pe, Fr, Sc, and Ro. Two thirds of the images that came after 7 sec. following *15, 30, 45,* and *60* min. adaptation were those of Lu, Fr, Sc, Sl, and Ro.[11] On the other hand 9 *S*s always experienced the image before 7 sec. regardless of the length of adaptation. In short we may say that the time of appearance of the image altered but slightly with varying adaptations to darkness.

(*b*) *Duration of Image.* The average length of the images of *0* was 2 min. 45 sec.; of *1,* 2 min. 33 sec.; of *3,* 2 min. 16.25 sec.;

[11]To and Pe were not subjected to experiments involving adaptations of 15, 30, and 60 min.

of *5*, 2 min. 16.7 sec.;[12] of *7*, 2 min. 16.5 sec.; of *15*, 1 min. 55 sec.; of *30*, 1 min. 56 sec.; of *45*, 1 min. 54 sec.; and of *60*, 1 min. 48.5 sec. An increase in the adaptation from *0* to *3* accordingly affected the length of the image in inverse proportion. An increase in adaptation, however, from *3* to *7* altered the duration of the image only by a few tenths of a second. As adaptation was lengthened from *7* to *15* the images became shorter as in the first instance. When adaptation was increased from *15* to *30* and *45* the length of the image varied only by 2 sec.; and between the images following *45* and those following *60* there was a difference of 3.5 sec. If we consider only the larger steps, *i.e.* *0*, *1*, *3*, *15*, and *60*, we may conclude that there existed an inverse ratio between length of adaptation and length of the image. *Ss* whose images were relatively brief or long after *0*, *15*, and *30* experienced correspondingly brief or long images after *45* and *60*. Ed's and Wi's images, for example, lasted longer than the average image after every period of adaptation, while Gr's, Ra's, An's, Fr's, and Sc's were comparatively short in duration. Fr's images after every adaptation disappeared before 1 min. 10 sec. There appears to be no relation between the moment of appearance of *S*'s image and the length of its course.

(*c*) *Typical Color Changes.* After-images following all periods of adaptation may be marked off into stages according to the predominate colors in each. *Y* was the color reported most often first except after *45* and *60*, when *R* and *B* were noted more frequently. This *Y* appeared immediately in almost two-thirds of the images following *0* and *1*, in less than one-half of images following *3*, *5*, *7*, *15*, and *30*, and in one-third and fewer of images after *45* and *60*. Other colors that appeared immediately instead of *Y* were *R*, *B*, and *Bk* named in the order of their frequency. *P* and *G* appeared rarely at first. No definite relation has been established between the occurrence of these colors and the adaptation periods. From one-third to one-seventh of the images altered very soon after their appearance. Within 7 sec. after the first color appeared, after *0*, approximately four-fifths of the images were *Y* and the rest *R* with the exception of one *G* image. After other adaptations, on the contrary, there was no increase in the number of *Y*s present in the image; there was, however, almost twice the number of *R* images as those that appeared first. *B* and *Bk* gave place to *R* after a few seconds of time. In this first stage only one color was present in about one-half of the images. More frequently,

[12]Note a difference of 1 min. 18.3 sec. between the average lengths of images of present and earlier experiments both of which followed the same intensity and duration of stimulus, *op. cit.*, 565. This discrepancy may be explained by the fact that *Ss* in the present experiment were greater in number and less experienced in introspective analysis than *Ss* in the former one, or by the fact that adaptation to darkness was not as definitely controlled in the earlier as in the present experiment.

however, there was more than one color present after *0* than after *15* and longer adaptations. *Bk*, *B*, *R*, *Y*, and *G* were often experienced as halos or margins around the central colors.

As we follow its course we find that the image at approximately 35 sec.[13] changed in hue. *R* followed the first color almost universally, *i.e.* in nine-tenths of the images, after *0*, *1*, *3*, *5*, and *7*. About three-fourths of the images of *15* were *R*; while after longer adaptations only a little more than one-half were *R*. In the images where *R* did not appear *Bk* was most often experienced. *B*, *P*, *Li*, and *G* were also noted occasionally after long periods of adaptation. The hue in the second stage was apparently not affected by that of the preceding one, for *R* after the first *Y*, *R*, or *B*, was the color most often reported. As *R* appeared in the images following *0* to *7* it often surrounded the *Y* which became smaller and faded away leaving the *R* in its place. Although this occurred in one-half of the images after *0*, in not more than 8 observations following other adaptations was it reported. This fact indicates that the images changed quickly after adaptations as long as 7 min. The majority of images in the second stage had, surrounding the central color, between one and three hues, the number of which did not alter with changes in adaptation. Adaptation likewise did not effect the hues of these surrounding halos which in all cases were *B*, *Bk*, *Y*, *P*, and *G*.[14] As the images of *0* were of longer duration than those following adaptation to darkness, so the second stage in the image similarly varied with adaptation. *R* disappeared after *0* at 1 min., 23 sec. upon the average, 22 sec. after the images following 7 min. of adaptation.

As *R* or the color in the second stage of the image often surrounded *Y*, so that color following the principal one in the second stage closed in upon it. *R* faded more slowly after *0* than after adaptations as *Y* did before it. This phenomenon is shown by the fact that *R* was reported as being present in the surrounding color in twice the number of images after *0* as after *15*, etc. After these adaptations *Ss* reported that *R*, etc., vanished quickly and the *Bk* came in its place.[15] *Y* remained in the center of the surrounding color in the few images when *R* was omitted. *Bk* was most often present in the third stage after all adaptations. *Bk*, however, was not so frequently present following *0* to *7* as longer adaptations as is shown by the fact that *Bk* appeared in one-half of the images of *0* to *7*, in three-fifths of those of *15* and *30*, and in seven-ninths and five-sevenths of those of *45* and *60*. Colors other than *Bk* that took the place of *R* were *B*, *GB*, and *G* and occasionally *P*. *B* and *P* were noticeably present after *0*. All images in this stage with the exception of 4 or 5 possessed one or infrequently two halos. *Y* was the halo experienced in about one-half of all images with the exception those of *60*. After this adaptation *g* and *Pk* were experienced almost as repeatedly as *Y*. Other halos around the *Bk* or *B*, etc., were *LiG*, *LG*, *g*, *Pk*, and a *B* which was seen rarely after shorter adaptations. A complementary relation frequently existed between the central color and its halo; for *Bk* was surrounded by the complement *Li* and where 2 halos were present around the central image the *Y* halo appeared within a *B* or *B-P*, and the *G* within a *P*.

After the third color sequence almost one-half of *Ss'* images did not change in hue.[16] About 36 of the 66 images following each adaptation altered at least once after *Bk* or the third color, and approximately 9 of

[13] This time varied proportionally with the length of the period of observation.

[14] *Ss* reported a *YG* or *Pk* halo around the perceived cross. The halo of the image was probably a result of this phenomenon.

[15] *R* remained in the center of *Bk* or its surrounding color from 10 to 55 sec.

[16] Fewer of these alterations in color occurred in short rather than in long images.

the 36 changed two or more times.[17] Many of these changes however were only variations in halos. *Bk* or *g* with just enough *Li* or color around it to make it distinguishable was continually experienced in the latter part of the image.[18] The halos around *Bk* were usually *Li-g*, *Li-B*, *Pk*, or *Li-G*.

The image, therefore, faded away as *Bk* with a little *Li-g*, or colored haze against the darkness of the field of vision. Colors near the end were experienced in a few more images following *0* than adaptation periods.

When the principal color decreased in saturation it also darkened in tint regardless of adaptation. The halos or surrounding colors, however, often decreased in chroma with no alteration in tint, as they were described as remaining *Li* or as becoming *Li-g*. Images following *0* began to lose their color between 1 min. 30 sec. and 3 min. After *1*, *3*, *5*, and *7*, 55 of every 62 images began to diminish in chroma between 1 and 2 min. upon removal of the stimulus. Succeeding longer adaptations to darkness a few *Ss* experienced grayish colors in the very first stage of the image but the most of their images darkened and became of poorer saturation at and after 30 sec. With one or two exceptions, after the hues began to decrease in saturation and darken in tint they continued hazier and darker until they finally vanished.

On comparing the images with the colors in the charts *S* usually indicated that after *0* the first *Y* was like '3' or '13;' after *15* it was '2' or '3;' and after *60* usually '2.' The '2' is somewhat grayer than '13' and much grayer than '3.' The *Li* halo in the latter part of the image was like '2' or grayer than '2.' The latter part of the image, *S* noted, was of poorer saturation than the first part. *R* in the second stage was like '9,' a *R-O*; '10,' a *Li-R*; or '11,' a vivid *R*; and when it was reported as growing darker it was '32' or '33,' a less saturated and darker color than the others. The vivid *R*, '11,' was more uniformly indicated after *0* than after adaptations. The *B* in the cross was '19,' a well saturated *B*, and that in the halo was a *B-G*, '21,' or a *Li-B*, '20,' *P* or *L* was between '39,' a color of medium tint and saturation, and '26,' a *Dk-G*.

The following introspective reports of *Ss* concerning the quality of image supplement the preceding section:

An "First *G* bright; other colors dark." *0.*
Fr "*Y* in *Bk* cross is grayer and duller when it comes back." *0.*
Ed "*B* border rich. First *B* that appeared with *Y*, was cold but not a dark *B*." *0.*
Su "*R* came in spots; first a *Li-R* that became darker. I saw these same colors, *R*, *Y*, and *G* on coming into the room from the light." *0.*
Sc "Colors glow like a flame. The edges are shining." *0.*

Ed "*Y* gets more distinct after a sec. or two. During stimulation I saw a double cross with *R* and *G* edges. *R* gets darker. *B* diamond around *R* darker than powder *B*." *1.*
Su "Long *B* strip and cross in it at first. I saw the same *B* strip when I came from other room as I had been looking out of the window." *1.*

Lu "Cross *Li* and then became darker." *3.*
Ne "Outline around *R* may have been dark *B* at first. It became *Bk*. *R* looked as if electric light were behind it." *3.*

[17] From 1 to 4 images following *15* and longer adaptations did not change in quality from the beginning to the end. There was only one change in 2 to 7 images following each period. About 14 times as many images of *60* and of *0*, moreover changed only once.

[18] *B* and *G* spots were occasionally reported instead of *Bk*.

An "*B* became darker all time till it faded." *5.*
Lu "*Pk* halo like an electric light." *5.*
Oa "Only a dark mass at the end." *5.*
Ra "Halo around *Bk* seemed like a dim electric light." *5.*

Wi "Vertical bar became *Bk* first while the rest was still medium *R*." *7.*
Ha "*B* halo seemed like gas flame." *7.*

Ha "Cross and background became indistinct." *15.*
Wi "*Pk* background gets duller." *15.*
Co "Everything dissolves into haze." *15.*
Sc "*Y* sun glow around *Bk*." *15.*
Fr "*Bk* gets dimmer in *Y* background." *15.*
Lu "*R* not like paper. Like electric light." *15.*

Ab "Colors not like colors on chart. They are more like electric lights." *30.*
Co "*Bk* bars melt into *Y* haze." *30.*
Ed "Rosy background gets faint and muddy before it vanishes." *30.*

To "Colors get mottled and muddy after a few minutes." *45.*
Co "Ray of light over it and it all goes." *45.*
Sc "*Bk* is faint like a shadow against the deep shadow. *G* a 'flat' color." *45.*

Lh "The darker the cross, the lighter the halo." *60.*
Ne "*B* gets dimmer." *60.*
Pe "*Y* wavery and faint near the end." *60.*
Ab "*Pk* background gets lighter as cross gets darker." *60.*
Sc "Image blurry near the end." *60.*

The after-images proceeding from all periods of adaptation, therefore, were similar in the following respects: they could be generally divided into periods, in which one color *Y*, *R*, *Bk*, or *G* or *B* predominated, a complementary relation existed between the tint of the central part of the image and its halo but not one between the hues, the image decreased in saturation and at least the central part of it darkened in tint. The images, especially those after brief adaptations, were often described as being like "lights" not as flat or surface colors.[19] Adaptation influenced the quality of the image in a comparatively small degree. The first *Y* was most constant and had more halos around it after *0* than after adaptations. The number of images in which *R* was present decreased as adaptation increased. Colors changed more quickly after *15* and long adaptations than after *0* to *7*. *Bk* increased in prominence and the chroma decreased earlier with increases in length of adaptation.

(*d*) *Fluctuations.* There was a little correlation between the length of adaptation and the number of fluctuations in the image. For although there was no difference between the number of

[19]M. F. Martin, Film, surface, and bulky colors and their intermediates, this JOURNAL, 33, 1922, 451.

them following adaptations of *0* and *7* there were about twice as many vacillations following *0* as *15*, *30*, etc. After *0* furthermore the greatest number of these occurred between 1 and 2 min.; after *15*, *30*, etc., between 30 sec. and 60 sec. These fluctuations were more generally scattered throughout the image after *0* and *1*, than after long adjustments to darkness. The individual differences in regard to this characteristic of the image are marked. For example, Ed, in *27* images, experienced 10 fluctuations, one to every 2 or 3 images. Pe, on the other hand, reported on the average 4 fluctuations with every image. Ra experienced approximately 5 changes during every image, while several *S*s seldom reported any vacillations.

(*e*) *Shape and Size.* The varying adaptations had little effect upon the shape of the after-image. After *15* and longer periods in one-third to one-half of the images the halo appeared a few seconds before the cross, while after *0* the background appeared first in only one-sixth of them. With few exceptions[20] the imaged cross was the shape of the perceived one. As the image progressed it was often thinner and sometimes "pointed at the ends." Occasionally a part of the cross, *i.e.* one or two 'bars,' vanished irrespective of adaptation. After *60* the cross in more than four-fifths of the images vanished between 10 sec. and 1 min. before the halo did, after *15* the cross vanished previous to the halo in one-half of the images, while after *0* it did so in less than one-twelfth of the images. The shorter the adaptation, therefore, the more persistent was the cross in its halo.

The size of the cross which was mentioned in two-thirds of the images was rarely observed as larger than the perceived cross. The first cross was reported as smaller than the perception in one-sixth of the *S*s' images after *0*, and in one-third of them after *15*, *30*, etc. The cross with its halos continued to diminish in size to the end of the after-image. After all adaptations only 4 images were reported as increasing in size and 2 as remaining the same size as they were in the beginning of the series. After long adaptations, however, the imaged cross decreased earlier than after short adaptations. For instance, between 30 and 60 sec. one-fourth of the images decreased after *0*, one-third after *15*, and one-half after longer adaptations. The other images began to noticeably decrease between 1 min. and 1 min. 30 sec. after the beginning of the image. Some descriptive phrases used by *S*s concerning the size of the cross are "shrinking," "thinner," "getting smaller and irregular," "thinner and pointed," "about one-half the sensation now." When the image became very small the darkness was often reported as "swallowing it up" or "casting a shadow over it," The

[20] An *S* occasionally described the image as 'fatter' or 'longer' than the perception. Pr often experienced the first cross of the image with round balls on its ends.

cross accordingly was especially constant after *0* adaptation and fairly constant after any adaptation; it was the size of or a little smaller than the perception when it first appeared and decreased as the image continued with more diminutions appearing earlier after long adaptations.

(*f*) *Position of Image.* Practically nothing can be said of the position of the image independent of its movement since it was reported in only one-ninth of all the images. When the position was noted the image was always described as being far away and very small in size. One *S* said that it seemed "on the level with my eyes" and another that it was "far away and not clear."

(*g*) *Movement and Rivalry of Color.* Movement, which was noted in about one-half the experiences, was most pronounced during the second 30 sec. of the image. The image usually went "up and to the right." Twice as many times, regardless of adaptation, was it considered as going up rather than down, and only 2 times did a *S* note that it went to the left. Sometimes Ed reported that she was looking over her shoulder. Once Su reported that when she kept her eyes closed the image did not move. Ed on the contrary, thought it make no difference in the image whether her eyes were closed or opened.

In practically all after-images regardless of adaptation, colors were reported as moving from the periphery to the center. *R* seemed to move in quickly or slowly on *Y* and in turn *R* was consumed by *Bk* or a color around it. The following introspections after various adaptations indicate this fact:

Ba "*Bk* goes in over *Pk*." *0.*
Wi "*Dk* outline eats *R* up." *0.*

Gr "*G* cross always comes from sides and bottom." *1.*
Ha "*R* first came in from points, met in center and poured out." *1.*

Ed "*R* moves in and absorbs *Y*." *7.*

Ri "*Bk* moves on *R* and makes it darker." *30.*
Sl "*P* thickens over *R*, *Bk* comes in over it from the sides like a curtain." *30.*

Co "*Y* halo covers up cross." *45.*
R "*Bk* comes in till covers up whole cross." *45.*
Sl "*R* comes in over *Y* cross." *60.*

Conclusion

In conclusion we note that to a small degree the length of adaptation influenced the flight of colors, qualitatively and quantitatively.

(1) As adaptation was increased the number of images in which *Y* and *R* were experienced decreased and those in which

Bk occurred increased. All colors likewise possessed fewer surrounding halos and changed more quickly with increase in adaptation.

(2) As adaptation was lengthened the number of images that appeared immediately decreased as did the length of the images.

(3) The fluctuations were more numerous and more rapid at the beginning of images following decreasing adaptations than following increasing periods.

(4) The cross appeared after the halo did and vanished before it more frequently, with increase in adaptation. Furthermore, as adaptation to darkness was lengthened the image decreased in size at an earlier stage in the process.

(5) Adaptation apparently did not affect either the relative amount of movement or the absolute position of the image.

EFFECT OF SERIAL POSITION UPON MEMORIZATION

By EDWARD S. ROBINSON and MARTHA A. BROWN, University of Chicago

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I. PROBLEM

The readiness with which an associative connection is formed depends upon its position in any series of which it may be a part. To this principle of memorization a substantial number of experiments lend support. In general, items appearing very early or very late in a list show a relatively high rate of learning, while items appearing between these extreme positions show a relatively low rate. Regarding the advantage of late position there is almost universal agreement. The advantage of early position, however, is not so certain. Welsh and Burnett,[1] for example, claim that primacy is really a specious facilitator of learning and that the first item is better learned only because there is a chance of rehearsing it while the other items are being presented. The factor of primacy would thus reduce to that of frequency. Woodworth and Poffenberger[2] see the advantage of primacy residing in the fact that the earlier part of a series suffers relatively little from proactive (associative) inhibition. It is apparent, therefore, that the effects of serial position upon memorization still constitute something of an issue. There are, furthermore, subsidiary problems, such as the relation of the total length of the memorized material to the importance of the positions within it, which have scarcely been touched.

In the course of two recent investigations[3, 4] in the Chicago laboratory there have accumulated considerable data bearing upon this question of serial position. We have results for two kinds of material and for a number of lengths of each. It is our

[1] G. R. Welsh and C. T. Burnett, this JOURNAL, 35, 1924, 396-401.
[2] R. S. Woodworth and A. T. Poffenberger, *Textbook of Experimental Psychology* (mimeographed edition), 1920, 71-72.
[3] E. S. Robinson and W. T. Heron, *J. Exper. Psychol.*, 5, 1922, 428-448.
[4] E. S. Robinson and C. W. Darrow, this JOURNAL, 35, 1924, 235-243.

main purpose to present and discuss these data. But first it seems advisable to bring together from the literature the principal results hitherto obtained.

II. Earlier Findings

Experiments dealing with serial position in relationship to memorization may be divided into two distinct types: (a) those in which the order of testing the various associations is definitely controlled, and (b) those in which the subjects are free to recall the memorized items in any order. Of course there are other differences in experimental procedure which have affected specific results, but we believe that this difference is the most important.

(a) *Experiments with Controlled Recall:* Miss Calkins[5] used the method of *paired associates*; 7, 10, or 12 pairs of colors and numbers were presented. Each color was exposed for 4 sec. and its numeral which followed immediately was likewise exposed for 4 sec. There was an 8-sec. interval between pairs. At the close of a series the colors were again presented but in altered order. The subject wrote down the 'suggested' number as every color appeared. The general procedure employed by Miss Calkins will be sufficiently clear if we consider her description of her first group of experiments: "some one color appeared several times in each series, once in an unimportant position with any chance numeral, but also once or more in some emphasized connection—either repeatedly with the same numeral (a 'frequent' combination), or at the very beginning or very end of a series (cases of 'primacy' and of 'recency'), or with a numeral of unusual size or color (an instance of 'vividness')." Thus she was able to determine the strength of the so-called emphasized connections as compared with that of the unemphasized connections. Our own interest is merely in the results for 'primacy' and 'recency.' She found that 'primacy' shows a certain advantage over chance positions where the total list is relatively short. But even in such a list 'recency' is a more important factor. With long lists 'primacy' is ineffective.

W. A. Owens[6] conducted an experiment which had as its purpose the critical evaluation of certain of the conclusions drawn by Calkins from the work we have just cited. According to Owens, Miss Calkins went further than her facts justified in interpreting the high value of recency in terms of time interval between memorization and test. He shows that a final association of a series may be learned readily for either of two reasons or for both. The final association may be tested sooner after it has been formed than are the other associations. But even if this were not true, the final association might benefit simply by being in an emphasized position in presentation. Our interest is mainly in this question of the value of various presentation positions. In regard to it Owens' data are quite convincing. Like Calkins, he used a form of the method of *paired associates*. Seven pairs of colors and numerals or nonsense syllables and numerals made up his lists. Unlike Calkins, he used a single syllable or color only once in a single list. His results regarding position are not, therefore, based upon the double possibility of association with a single color or number, but merely upon the learning values at different positions. This seems to us a simpler and more straightforward method than that of Calkins. True, it assumes that the different syllables or colors are, apart from position, of the same difficulty, but this assumption is readily justified if massed results are considered. In order to have some basis for distinguishing the

[5] M. W. Calkins, *Psychol. Monog.*, 1, 1896, No. 2, 35-56.

[6] W. A. Owens, unpublished Master's thesis, University of Chicago, 1911, typed copy in the University library.

factor of time interval between learning and test from that of serial position, Owens calculated the average time interval for each serial position. The following table includes those of his results which are most relevant to our discussion:

TABLE I

Average of Entire Experiment in which 11 *Ss* Memorized Twenty Lists Each

	Position						
	1	2	3	4	5	6	7
% Recalled	39.3	34.5	36.3	35.5	34.1	37.1	60.0
Time in sec. between learning and recall	76.8	67.6	58.0	48.6	38.0	26.8	20.4

The final position shows the outstanding advantage, though it is clear that advantage may well be the result of the relatively short interval which, on the average, separated the learning and the test at this serial position. The first position shows only a slight advantage, but when one considers that the time interval in this case is markedly the longest, there seems good reason to believe that the actual advantage of this position is somewhat greater than the results directly show it to be.

Ebbinghaus[7] reported an experiment in which the time interval between memorization and test was the same regardless of serial position in presentation. He used the well known *prompting* or *anticipation* method in which the items of the memorized list must be recalled in original order. When failure occurs at any item, that item is displayed to the subject. The advantage of this method is that a test trial is also a further learning trial. The results in terms of number of promptings (an inverted measure of efficiency) are presented below. They are based upon a number of lists of 10 and 12 nonsense syllables. Ebbinghaus was the subject throughout.

TABLE II

	Serial Order											
	1	2	3	4	5	6	7	8	9	10	11	12
No. promptings												
10 syll. series	0	3	6	9	23	24	31½	25	23	5½		
12 syll. series	0	11	21	13½	35	36	36	29½	43	37½	34	11

With both lengths we here find a strong advantage at both the beginning and end of the series. These results depart from those of Calkins and Owens in that they show a greater advantage for the early than for the late positions. In so far as we are interested in the effects of serial position, this departure is worth taking into account, because the Ebbinghaus experiment is, after all, the first among those mentioned that, by establishing equal, or nearly equal, time intervals between learning and testing of the various items, approaches direct measurement of the importance of serial position.

Warden[8] conducted an experiment in the field of motor learning similar to that of Ebbinghaus. Stylus mazes were learned by human subjects. Two patterns were employed and each pattern was learned with either the right or the left hand. Each of these four conditions was represented by 15 *Ss*. In maze learning the efficiency of the *S* after one or more trials is tested by putting him through another trial under conditions which are essentially like those of the learning itself. Thus the order of learning is also the order of testing and one element in the complex act is not likely

[7] H. Ebbinghaus, *Grundzüge der Psychologie*, 1902, 1, 624-626.

[8] C. J. Warden, *J. Exper. Psychol.*, 7, 1924, 98-116.

to show to advantage because of the benefit accruing from a shorter time interval between learning and testing. This is the reason why we consider Warden's experiment importantly similar to that of Ebbinghaus.

Warden divided his mazes into three sections: (1) the first three cul-de-sacs; (2) the next four cul-de-sacs; (3) the last three cul-de-sacs. Those of his results that bear most directly upon the present problem are in terms of the mean number of cul-de-sacs entered in different sections of the mazes during the complete acquisition of this habit and in terms of the mean number of trials required to eliminate cul-de-sacs in these successive sections.

TABLE III

	Section		
	First	Middle	End
Av. cul-de-sacs entered	11.2	23.3	16.9
Av. trials required to eliminate cul-de-sacs	16.4	34.8	23.7

Here again the merits of the first and final serial positions are clear and, in agreement with the Ebbinghaus results, the first positions are better than the final ones.

Finkenbinder,[9] who used a form of *prompting* method in connection with his study of the curve of forgetting, has reported that: "the initial and final syllables of the series were the first to be mastered; the central section of the series, usually the seventh or eighth syllable, was the last to be learned." He does not, however, give any quantitative results along this line. Neither does he mention any difference in rate of learning between the first and final positions.

(b) *Experiments with Free Recall:* In a number of the experiments upon the effects of serial position there has been no control of the order in which the various items in the list have been tested. The *S*s, after the presentation of the material, have simply been instructed to recall as much of it as possible. Before considering the individual experiments of this type, we shall point out one result that is naturally to be expected from them. If, at the beginning of the process of recall, *S* is not required to recall in any set order—for instance in the order of learning—there is a strong likelihood of his first recalling the item most recently observed, which is of course the one occupying the final position. When this occurs the final item is protected against being forgotten, while the earlier items in the list have more than a fair chance of being forgotten. Thus, in experiments of this type there should be a greater advantage for finality than its rôle in learning warrants and also a smaller advantage for primacy than its rôle warrants.

Bigham[10] employed 10 types of memory material: (1) visible numbers, (2) audible numbers, (3) visible colors, (4) audible colors—color names, (5) visible geometrical forms, (6) audible names of forms, (7) visible words, (8) audible words, (9) visible nonsense syllables, (10) audible nonsense syllables. The *S* was presented once with a list of one of these types and then required to recall what he could remember. The results were treated in such a way as to throw light upon a number of problems, among them that of serial position. In this case the data seem to be the result of pooling the results obtained from 6 *S*s performing under all 10 conditions. Three measurements of efficiency are given for each serial position. They are *misplacements*, *forgetting*, and *insertion of wrong words*, all of them being inverse measures. The following table presents these values as determined from tests made 2 sec. after presentation.

[9] E. O. Finkenbinder, this JOURNAL, 24, 1913, 8-32.

[10] J. Bigham, *Psychol. Rev.*, 1, 1894, 453-461.

TABLE IV

Serial Position

	1	2	3	4	5	6	7	8	9	10
Misplacements	2.5	7.5	11.7	12.5	15.7	12.1	10.8	11.3	6.3	2.5
Forgetting	5.8	11.3	15.9	23.8	20.4	14.6	23.8	14.6	12.1	6.7
Wrong words	3.3	1.3	1.7	2.1	1.3	0.4	0.8	1.3	0.8	0.8

According to Misplacements and Forgetting the first and final positions show about the same advantage over the other positions. There is little to be said about the insertion of Wrong words except that it is not at all closely correlated with the other types of efficiency. Perhaps it is natural to expect more errors of this sort where efficiency is in other respects relatively high.

W. G. Smith[11] employed lists containing 10 nonsense syllables which were visually presented at a rate varying with the desires of the *Ss*. Presentation was visual and recall was written. Results are given where memory was tested after 1, 3, 6, 9, and 10 presentations. Eight *Ss* served in the experiment. The following table brings together the findings regarding serial position.

TABLE V

Percent Recalled after Different Numbers of Presentations

Number of Presentations	Serial Position									
	1	2	3	4	5	6	7	8	9	10
One	81	52	24	16	16	24	26	26	62	84
Three	84	67.5	39	38	34	33	29	44	69	92
Six	81	61	42	42	34	32	46	54	74	85
Nine	89	67	49	41	32	33	48	64	77	93
Twelve	92	58	46	41	56	57	57	61	84	91

Again we see that both the first and final positions are advantageous for learning and again we have a case in which the difference between them in this respect is very slight. Fortunately Smith recognized that the order of recall might be important. He tells us that, though the first two syllables were usually recalled first, the last two as a rule came next. This means that on the average the time between memorizing and test was shorter for the last two than for the first two syllables. As we have already pointed out, there would thus be a tendency to exaggerate the merely positional advantage of the final syllables. If in this experiment of Smith's the syllables had been recalled in original order so that the time interval would have been of the same magnitude whatever the serial position, it is quite possible that the first positions might have proved to be appreciably more advantageous than the final positions.

Woodworth and Poffenberger[12] describe an experiment performed at Columbia in which lists of 10 digits were read to a class of 26 students. In all, 10 lists were used. Immediately after the reading of a list the *Ss* attempted to reproduce it in writing. The results follow:

TABLE VI

	Serial Position									
	1	2	3	4	5	6	7	8	9	10
% of Error	1	10	17	23	27	24	24	19	13	2

[11] W. G. Smith, *Psychol. Rev.*, 3, 1896, 21-31.

[12] R. S. Woodworth and A. T. Poffenberger, *op. cit.*, 71-72.

Woodworth and Poffenberger point out that the relative advantage of the first position is not quite so great as in the Ebbinghaus results, but they do not mention that the order of recall was fixed in the latter case, a fact that may easily account for all of this small difference.

Three experiments on serial position have recently been reported from the Bowdoin laboratory. The first, conducted by H. W. Lamb and described by Welsh and Burnett,[13] was an attempt to test the laws of primacy, frequency, recency, and vividness. Nonsense syllable lists of 8 items each were employed. Six *Ss* were presumably presented with 8 lists during the course of the experiment. The presentation was serial and visual. After the exposure of a list *S* recorded what he could remember of it. The recall values for all 8 positions are not given, but the relative values of primacy and recency are as 44 and 65 respectively. M. W. Waterman repeated this experiment two years later with 11 *Ss*. He obtained a reversal of the relative advantages of primacy and recency. The values obtained for these two factors were as 64 and 55 respectively. A similar, but somewhat more extensive experiment was conducted by Welch and Burnett. Twenty-six *Ss* were employed. In this last experiment there was an important innovation. The *Ss* were given specific instructions not to rehearse the earlier syllables during the presentation of the later ones. The relative values of the different serial positions are given as follows:

TABLE VII

	Serial Position							
	1	2	3	4	5	6	7	8
Recall Scores	30	34	37	27	31	29	59	142

Welch and Burnett are quite convinced that their instructions against rehearsing the earlier syllables during the display of the later ones is the main explanation of their failure to show any degree of advantage for the first syllables. Of course this may be true, but they tell us quite frankly that their *Ss* often wrote the last syllable immediately upon being given the signal to write. Their experiment is, therefore, open to explanation in terms of the greater time interval between memorizing and recall in the case of the first syllables. Welch and Burnett have somewhat rashly presumed that the advantage of primacy as it has been shown in the past rests upon a failure to prevent rehearsals of the first syllable. But their discussion of the history of this problem is based upon second or third hand sources, namely three elementary textbooks and one elementary manual of experiments. If they had gone to the sources they could hardly have missed the fact that Smith's experiment showed a substantial advantage for the first syllables, even though the *Ss* were required to read each syllable aloud during presentation, a procedure which it seems to us, was a better guarantee against rehearsal of the first syllables than the instructions upon which Welch and Burnett put such faith, especially when it is remembered that Smith's exposure rates were all faster than one syllable a second.

Certain observations on casual memory reported by Galton and by Calkins[14] show something of the importance of primacy. These workers recorded the relative numbers of childhood and recent associations. Professor Calkins has included the results of both studies in the following simple table.

[13] G. B. Welch and C. T. Burnett, *op. cit.*

[14] M. W. Calkins, *Psychol. Rev.* 5, 1898, 461-462.

TABLE VIII

Records of	Number of subjects	Percent Childhood Associations	Percent Recent Associations
Galton	1	39	15
Wellesley Studs.	90	14.7	32.7
Old and Middle-Aged Persons	87	33.4	30.9

Obviously there were many other factors aside from serial position effective in determining the distribution of these reminiscences, but the facts do, nevertheless, point strongly toward the importance of primacy.

Summary of earlier findings. Items occupying an early or late serial position clearly have an advantage over those in an intermediate position. All of the experiments cited in which a comparison is possible between learning of items late in the list and those in middle positions, show that the late items are learned most readily. Because these late items are often tested sooner after they have been learned than are other items, the advantage which they seem to show may be somewhat exaggerated. The fact, however, that finality appears to advantage in the experiments of Ebbinghaus and Warden seems to indicate that the final positions have a certain merit even when they are not favored by less chances of being forgotten. The advantages of primacy are not shown quite so universally as are those of finality. Welch and Burnett found no advantage for primacy and Calkins found that this factor was of no importance in learning long lists of material. But in most other cases the advantages of primacy are clear and occasionally they show up more clearly than those of finality.

III. SOURCES OF OUR DATA

In the first of the two experiments from which our own data are drawn the materials memorized were lists containing 6, 9, 12, 15, or 18 nonsense syllables. The *S*s had previously had several experiences in learning nonsense syllables. In our second experiment the memorized materials were lists of 4, 6, 8, or 10 three-place numbers.

The method employed in the learning was a form of the prompting method which is sometimes spoken of as the method of *anticipation*. The list was presented serially, 2 sec. per item. After the first presentation of a series as a whole and upon the presentation of the first item, *S* tried to spell the second item. When the second item appeared, he tried to spel the third item. When the third item appeared, he tried to spel the fourth, and so on. The same procedure was followed until *S* could go through the entire list without an error or omission. During every presentation after the first a record was made of the syllables which were correctly anticipated within the given time (2 sec.) and of those upon which *S* failed. The responses were scored either *right* or *wrong*. There were no partial credits. That item which was actually first in the list did not receive a score, because it was used merely as a cue. Thus we obtained a very complete picture of the progress of the learning, not only for the lists as wholes, but also for the separate items.

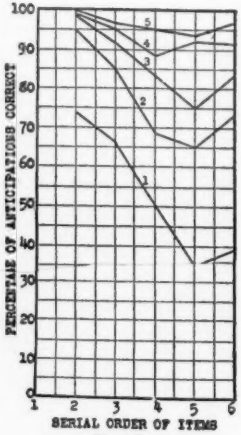

FIG. I
SIX-SYLLABLE LISTS

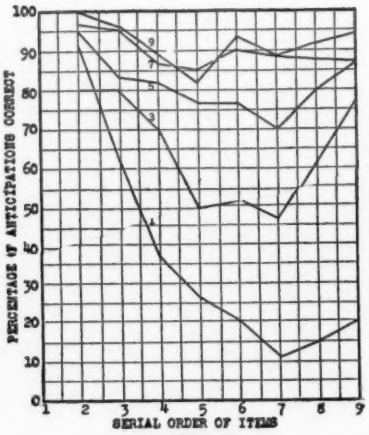

FIG. II
NINE-SYLLABLE LISTS

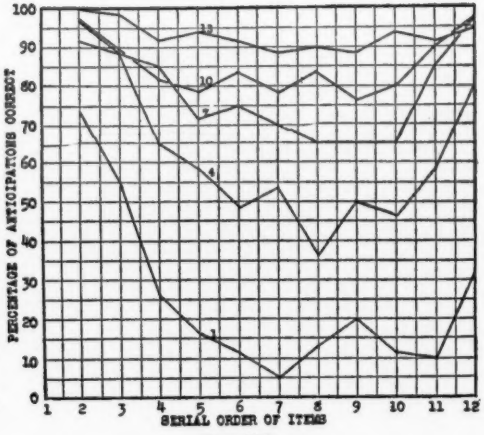

FIG. III
TWELVE-SYLLABLE LISTS

NOTE: The numbers on the curves indicate repetitions.

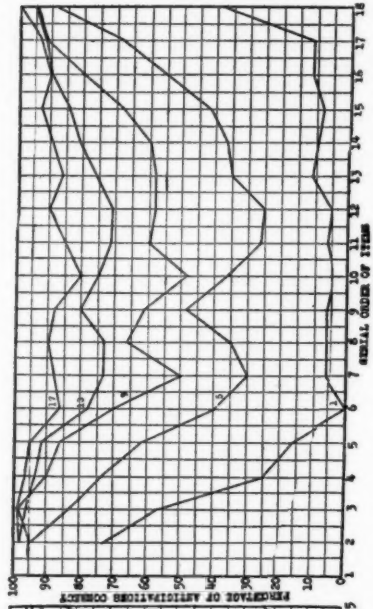

FIG. V.
EIGHTEEN-SYLLABLE LISTS

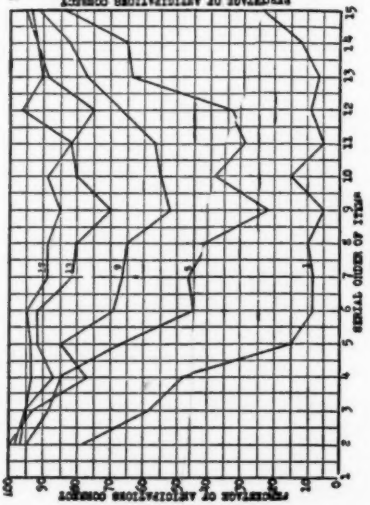

FIG. IV.
FIFTEEN-SYLLABLE LISTS

NOTE: The numbers on the curves indicate repetitions.

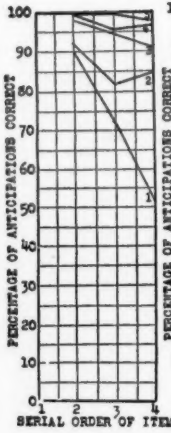

FIG. VI.
FOUR-NUMBER LISTS

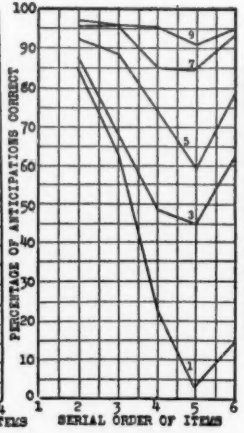

FIG. VII.
SIX-NUMBER LISTS

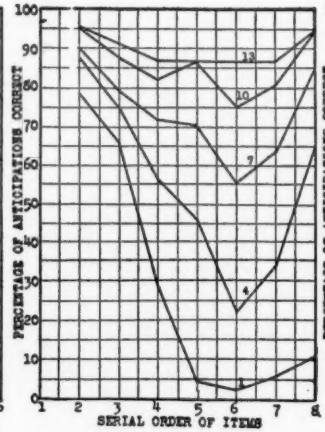

FIG. VIII.
EIGHT-NUMBER LISTS

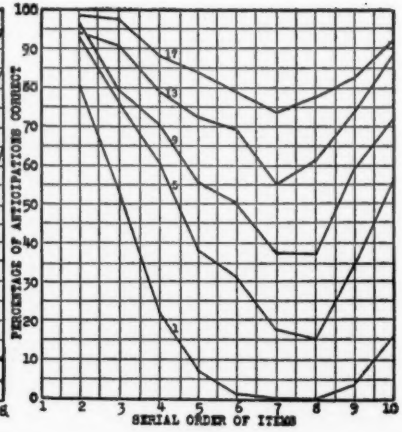

FIG. IX.
TEN-NUMBER LISTS

NOTE: The numbers on the curves indicate repetitions.

IV. Effect of Serial Positions In Lists of Different Length

Figures I to IX show the percentage of correct responses for each item during the various repetitions. Every curve for the syllable lists is a composite of 60 performances. Ten *S*s learned 6 lists of every length. Thus a percentage value in one of these curves of 91.7 would mean that of 60 attempts to anticipate a syllable in a certain position in a list of a certain length 55 were correct. Every curve for the number lists is a composite of 88 performances.

Eleven *S*s learned 8 number lists of every length. We are presenting these results in graphs because we feel that the rather extensive data involved are more readily surveyed in that form. The numerical values have been tabulated elsewhere[15] and we do not believe it necessary to present both the graphs and tables. Not all of the repetitions involved in the complete learning of every length of list are represented. If they were, nothing of significance would be added and the facts would simply seem more complex than they really are. We do not believe that there is any reason, however, why the method of sampling which has been employed does not give a very ample picture of the facts as a whole.

That primacy and finality are both favored positions is clearly evident in this set of figures. Usually there are a number of middle positions which seem to be at a distinct disadvantage, although in the first stages of learning in the case of the shortest lists this is not true. In the 6-syllable lists as observed after one presentation there is only one syllable, the fifth, which is less thoroughly mastered than the last. In the four-number lists as observed after one presentation there is a steady decrease in advantage from the first to the last position.

In our experiments primacy is consistently a stronger factor in favoring learning than is finality. We do not believe that this is due to rehearsal of the first few syllables during the presentation of the later ones. *S* actually recited the items aloud, just before they appeared, if possible, and, if not, as soon as they appeared. In the syllable lists the separate letters were recited and in the number lists the separate digits were, of course, recited. This fact gives fairly good assurance that *S* was engrossed with that part of the list which happened to be before him at any given time.

The primacy factor also affects more positions than does that of finality. Judging again on the basis of the first repetition, in the syllable lists between the first 4 and the first 6 items show a benefit from their early position. In the number of lists

[15] Martha E. Brown, Master's thesis, University of Chicago, 1925, typed copy in the University library.

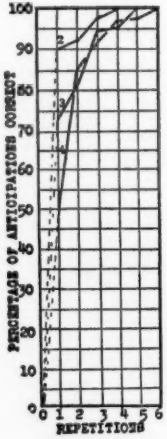

FIG. X.
FOUR-NUMBER LISTS

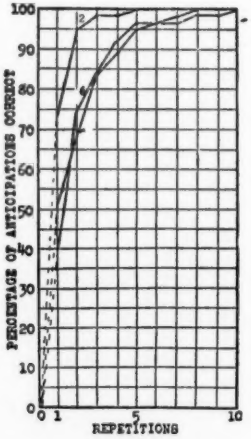

FIG. XI.
SIX-SYLLABLE LISTS

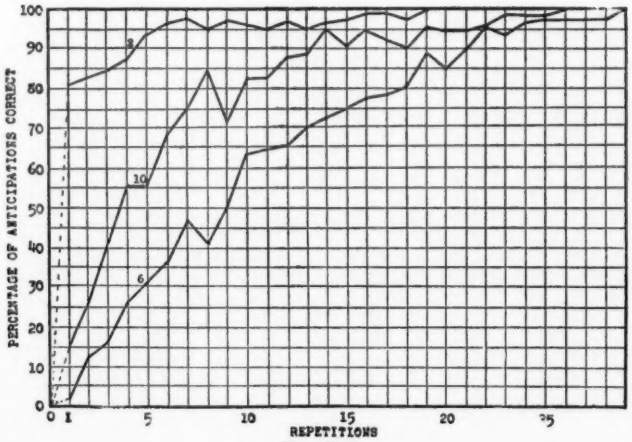

FIG. XII.
TEN-NUMBER LISTS

NOTE: The numbers on the curves indicate item in list.

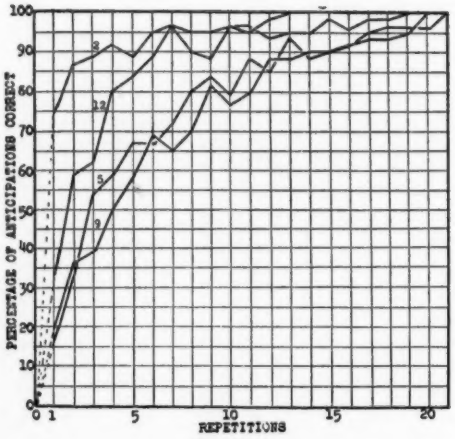

FIG. XIII.
TWELVE-SYLLABLE LISTS

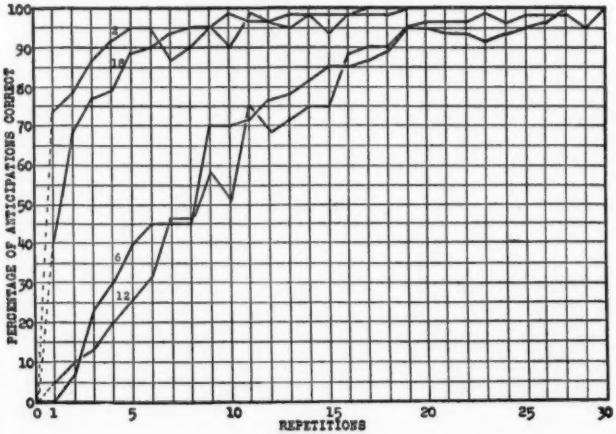

FIG. XIV.
EIGHTEEN-SYLLABLE LISTS

NOTE: The numbers on the curves indicate item in list.

from 3 to 5 of the earlier items show such a benefit. When the lists become subjected to more and more repetitions the decreasing of the primacy and recency effect, as we consider later, naturally becomes less marked. In the first repetition of both types of lists the maximum number of positions included in the finality effect is two. With the progress of learning the finality effect takes on a somewhat wider scope in the 15- and 18-syllable lists, but in practically all instances the scope of the primacy effect remains greater throughout the learning.

V. Memorization Curves for Individual Items

So far as we are aware none of the investigators of the effects of serial position upon memorization has discussed the form taken by the memorization curve for individual items. There have been studies of memorization curves for lists as wholes,[16] but we feel that the course of memorization for individual items is also worth consideration. Because of the nature of our records we have been able to construct curves showing the nature of the process of memorizing as it shows itself in the various units of our materials. These curves are shown in Figures X to XIV. Because of considerations of clarity and space we are presenting only a small number of the curves furnished by our data. Those omitted, however, fall into the picture of the samples given.

The curves all take the negatively-accelerated form so typical of learning curves for all kinds of activities. The degree of negative acceleration varies with the serial position of the item in question. The second and last items in the lists show a much sharper deceleration than do those in the middle portions. And the deceleration shown by the second item is in practically all instances more marked than that shown by the last item.

Among the curves here presented there are three that manifest early positive acceleration. This is true of the sixth and possibly of the twelfth syllable in the 18-syllable lists and for the sixth number of the 10-number lists. In each of these cases the item showing this positive acceleration is situated in a relatively unfavorable position in a relatively long list. One can readily appreciate why early positive acceleration might be expected under such circumstances. If a list is very long there will be some items the learning of which can scarcely get under way at all until the learning task is simplified by the mastery of part of the list. Of course, if learning cannot begin at once in all parts of the list, we should expect it to involve at first only those items which are situated favorably, *i.e.* at the earlier and later

[16] C. L. Kjerstad, *Psychol. Monog.*, 26, 1919, Whole No. 116; E. S. Robinson and W. T. Heron, *op. cit.*; E. S. Robinson and C. W. Darrow, *op. cit.*

positions. After these items are, so to speak, able to take care of themselves, we should expect the learning of the less favorably located items to gather momentum.

VI. Summary

It is evident from the work of earlier investigators that both primacy and finality of serial position facilitate memorization. Except in the experiments of Ebbinghaus and of Warden, the importance of primacy has probably been somewhat undervalued and that of finality somewhat exaggerated. In our own experiments the advantages of both primacy and finality are pronounced; and primacy shows an even greater advantage than finality. We have also been able to show that this primacy effect extends in some degree over several items in the first portion of a list, while the finality effect is much more limited in scope. From our data we have constructed curves of memorization for representative separate tiems. In their general negative acceleration these curves are like most other curves of learning. The sharpness of deceleration varies according to the serial position of the item in question. The more favorably situated items show a sharper deceleration. There is evidence of early positive acceleration in the case of items which are unfavorably situated in very long lists.

THE BIOLOGICAL CONCEPTION OF LIBIDO

By K. C. Mukherji, Dacca University, India

The meaning of 'libido' is still open to controversy. The term was first used by Jung in the sense of primordial energy, but various interpretations of it have since been made. Freud has attached it to 'sex' energy. Alder defines it in terms of the 'will-to-power,' and Boris Sidis, adhering to Alder, interprets it in the sense of self-preservation. Bergson and McDougall have ascribed to it a biological significance and conceived it as identified with *élan vital* or vital energy. No one, however, has shown what it exactly means, how it is related to physical and neural energy, or wherein lies its specificity.

It is difficult to say in exact terms what energy is, but it is generally expressed in terms of work. The same kind of energy may play against different forces and perform different actions. So a mere difference in the work done can not justify the postulation of a difference in the essential characteristics of the energy involved. If it be accepted that physical energy, as expressed by Adrian,[1] is the only fundamental energy which is transferable into other forms—in accordance with the laws of thermodynamics—then it is obvious that what is neural or mental is nothing but the special manifestation of physical energy and that the conception of any other kind is quite unnecessary. But Adrian seems to forget that impulse studied in the isolated nerve may not be the same phenomenon as that occurring in the animal. Even if it were a fact—as it is not—that electrical manifestations are all that we know of neural energy, we should probably not be justified in saying that there is nothing but electrical energy involved in neural functions.

Adrian finds upon experimental investigation that every stimulation brings about a change in the surface layer of the fibres and a movement of ions into the field around it. This develops in the active region an electric current which is to be, according to him, identified with a nervous impulse. Of course, physical and chemical relations are undoubtedly present in the nervous impulse; but they may also be regarded as degradation of still higher forms of inherent organic energy unknown to us. Besides, Adrian seems to fail in taking the full view of the matter. He lays particular emphasis upon one aspect of the neuro-muscular activity; that is to say, the muscular contraction which is ungraded and susceptible of exhaustion. It is called

[1]E. D. Adrian, The conception of nervous and mental energy, *Brit. J. Psychol.*, 14, 1923, 121-125.

ungraded because the muscle-fibre or the nerve-fibre has been experimentally found to react to the stimulus according to the 'all-or-none' principle; there is no gradation of reaction according to the strength of the stimulus, though it is true that individual fibres may differ in their inherent sensitivity. Adrian's investigation may cover only this aspect of the neural activity; but he has totally ignored its other aspect, *i.e.* the maintenance of tone which is caused not by the consumption of available materials manufactured by the cells to provide for the intermittent flashes of activity—as in the case of contraction—but by the coöperation in an intact central nervous system of afferent impulses from antagonistic muscles. This adaptive process seems to develop a certain molecular 'set' in the living substance, a temporary static attitude which is produced and maintained by a fairly uniform environment and which may be regarded as involved in the neural law of reciprocal inhibition and facilitation. Here the physico-chemical explanation meets a failure and proves its own limitation. Besides the teleological ordering, as demonstrated in the extraordinary delicate regulation of breathing, in the fine discrimination exhibited in the excretory activities of the kidney and elsewhere, is still a mystery unless it is referred to the present-day vitalistic hypothesis. It far exceeds anything in the realm of physics and chemistry. Here lies the distinctiveness of the neural energy involved in the biological activities which not only differ in appearance but are dependent on fundamentally different processes from those known to us in the realm of physical chemistry.

But turning to the performance of mental energy we find that there are, analogous to the two varieties of neural activity, two varieties of mental action, one variety being explosive and intermittent and resulting from the need to make numerous judgments and decisions; the other adaptive and continuous, arising from the need to maintain a certain 'set' or mental attitude. Again, the peculiarity of mental function is said to lie in its purposiveness. But this element also is not entirely lacking in neural activity, taken simply on the physiological level. Qualitative selection of stimulus, modification of neural response by dispositions due to past events, and coördination of movement on special projection are marked even in purely reflex reactions. The two systems of energy, mental and neural, develop out of the same oösperm derived from the parental organisms; and in their development also they run parallel in complexity of function. The psycho-neural energy assumes a more mental form as higher and more plastic nervous areas are brought into function. Aphasia is a clear instance in this respect. Speaking of the mechanism of repression and displacement Freud says; "The mechanism of these processes is entirely

unknown to me. Anyone who wishes to follow up these ideas must try to find the physical analogies and prepare the way for a demonstration of the process of motion in the neuron."[2] This indicates that the neural process is very intimately associated even with the subtle workings of the mind. Besides, psycho-physical parallelism, the validity of which we do not care to discuss here, seems to stand upon the postulated identity of the two kinds of energy. So does the conception of a hierarchy of neural centres. McDougall, following in the footsteps of Leibnitz, conceives that "it may be that the soul that thinks in each of us is but the chief of a hierachy of similar beings, and that this one alone, owing to the favorable position it occupies (I do not mean spatial position), is able to actualise in any full measure its capacities for conscious activity; and it may be that, if the subordinate beings exercise in any degree their psychic capacities, the chief soul is able, by a direct or telepathic action, to utilize and in some measure control their activities."[3] The history of life is a striving towards individuality. What is a blind or purblind impulse in the humblest creature and what remains there forever below the conscious level assumes the form of conation and is expressed most clearly and richly in man's conscious nature. It means that the same psychergy develops and differentiates in complexity in course of biological evolution, reaching its climax in the human organism.

Taking all the facts together, it seems that the neural and psychic energy is produced as a consequence of the specialization which the biological energy (with which some would now consider 'mind' in the widest sense as co-extensive) undergoes in evolution.' So the psychic and neural energies are but the two differentiated forms of the biological energy, differing so far from each other in refinement that each is governed by its own laws and works in its own way. Furthermore they are so inseparably connected with each other that they constitutionally form a functional unit which expresses itself clearly in the performance of pure instincts as viewed from the standpoint which McDougall has reasonably held. It is this inborn neuro-psychic energy which is to be biologically conceived and conveniently used as 'libido.' All instincts are related to this libido and derive their functional energy from it. Now whether it is the common fund of energy for all instincts or whether it is more or less divided and its component quantities attached separately to different instincts, limited in their utilisation, is a question which is also variously answered. Freud has reduced the fundamental strivings of mankind to one, the 'sexual,' and

[2] S. Freud, *Interpretation of dreams*, 1913, 475.
[3] W. McDougall, *Body and mind*, 1911, 366.

has thus avoided the difficulty of partition of energy; but he believes in part-instincts, within the sexual, between which energy is interchangable. Janet analyses human tendencies and finds them to constitute a hierarchy. He conceives the psychological force as stored in various levels. The old elementary tendencies have much force at their disposal while the higher and more recently acquired tendencies have less. So the principal reserves of energy are, according to him, localised chiefly in deep-seated elementary tendencies. But a combination of the two views—one postulating a common fund of energy for all instincts and the other a non-interchangeable quantity of energy attached to each—seems to be most satisfactory.[4] McDougall also has assumed this position in his *Outline of psychology*.

[4]The Tantric conception on this point seems to be one sided and to lean toward a common fund of energy. The snake power or Kulakundalini Sakti of the Tantras—the ancient Hindu scriptures—is what is libido in terms of Jung; but it is localized in one particular center called Muladhar and regarded as the only source of all energy for some sixty odd vrittis or instincts. Cf. A. Avalon, *Tantras of the great liberation*.

THE MECHANISM OF CONSCIOUSNESS: IMAGES[1]

By F. Russell Bichowsky, National Research Council

The two preceding papers of this series have discussed the activity of the two lowest levels of consciousness: the pre-sensation (thalamic) and the percept (Rolandic). Above these levels, according to the view advanced here, lie at least two higher levels, the image (lobular[2]) and the judgment (frontal). This paper will be confined to the relations of the 'image level' to the lower levels.

The existence of images as distinct from percepts is one of the most patent of introspective facts; at least I have found that even first attempts at introspection almost invariably distinguish the vivid, external, projected, impersonal, concrete percept from the subjective and meaningful image, often of a different modality. Nevertheless, more careful introspection shows that, considered as pure 'feels,' the image and percept are puzzlingly alike, the sharp distinctions found by the more naïve observers depending, not on intrinsic differences of quality of the two types but on consciousness of certain unlike relations in which they commonly stand. When those factors which depend on relations between content and reference are ignored (the usual rule for laboratory introspection), it becomes very difficult to find tangible differences between the image and the percept. It is, therefore, not surprising that previous observers, working largely under instructions which required only the report of content and which by implication often depreciated the report of conscious relations, should have expressed doubt as to the existence of any content other than 'sensations.'

Thus my *Os* even when most carefully distinguishing the image from the percept would often pause to remark that they are "essentially of the same kind of feeling stuff." "If their particularizing qualities are abstracted they have the feel of pre-sensations." "The visual percept and the visual image are both visual. I can't distinguish them on that score. They are like pre-sensations too in that." Both percept and images, moreover, are individuals. They have limits and space and time qualities. In this they differ from pre-sensations. The image was thus said to be "definite," "to be projected in space and time." "It was a picture," "a tone," "clear cut" and "distinguishable from other images." "As X would put it, it could be analyzed into definite 'sensations.'"

[1]This is the third of a series of articles on the "Mechanism of Consciousness." The first, on 'Pre-Sensation,' appeared in the October 1925 number of the Journal (36, 1925, 588-596); the second, on the 'Percept Arc,' appeared in the July 1926 number (37, 1926, 382-390).

[2]This is the association-area of the physiologist. I have avoided this name so as not to confuse the anatomical and psychological categories.

In spite of these likenesses, however, and quite apart from many at least of the striking differences of relation often exhibited by images and percepts, my *O*s could distinguish images and percepts by the following apparently intrinsic differences.

(1) *Intensity and vividness.* Thus the image was said to be "less intense" and of "less vivid quality" than the percept. At first it might be thought that this difference of quality was due to the fact that the percept is a primary type of activity while the image is a more or less weak reproduction. But even where the image was generated in ways not involving the percept with which it was compared, my *O*s still maintained that the image "had an intrinsically less vivid feel than the percept."

This lack of vividness is apparently not solely a matter of the comparative strength of the underlying neural impulse, but is a qualitative distinction; for it is possible to find images which in a real sense of vividness are more vivid than ordinary percepts, sometimes even more vivid than the percepts generating them. Under these conditions my *O*s remark "that the enhanced image is really the more intense and bright but that there is a sort of qualitative brightness and vividness which the percept has and the image has not."

(2) *Subjectivity.* The image is subjective, mental, independent of external stimulation and of bodily position. The percept is external, real; its existence depends on external stimulation. Its character is independent of the position of the body. It is not private or personal.

In detail the image is said to be "unreal," to be an "internal thing," "not possessing the quality of concrete external reality," "subjective," "definitely located as a part of experience, not as a part of the external world," "it is not objective." More specifically, "the space of the image was not the same as that of the percept." "The image 'apple' was localized in space, but its space was not objective. It was independent of the position of my body, or of the movement of my eyes. It was an inner space." "The image was in its normal coloring. The percept had a uniform yellow cast." (The *O*, unknown to himself, was looking through yellow glass. The image had been stimulated by the way of audition. In case the image is stimulated by the appropriate percept it is usually toned to accord with the tone of the percept, but not always.) In some cases the orientation of space of the image was different from that of the percept; but again not always, particularly under normal conditions. The best tentative explanation of these anomalies seems to be that the space and time frame of the image is essentially independent of that of the percept; but that stimulation of the image by the percept tends to orient the space frame of the image to correspond with that of the percept. More study of this aspect of the problem is badly needed.

Even the time frame of the image differs from that of the percept level. My *O*s remark that though the image was in time it did not fill time. "Images have time sequence, some last longer than others; but there is no sense of absolute interval." "The image of the whole lecture which seemed to last the full period was present; but there was no accompanying perception of the lapse of more than a minute or so." "I can have the image of a whole epoch in a moment," one *O* reported, though he promptly corrected himself by saying, "I guess that's somewhat exaggerated. I have the sequence right enough. It seems a long period; but of course it doesn't really seem an epoch." Another *O* reported that "a real perceptual second

lasts a whole second no matter how many images I have in it. The image scale of time is artificial and relative, the perceptual real and absolute." This is a report from an *O* who had just undergone a series of tests on time-perception where the apparent length of the perceptual interval had been changed by imaginal multiplication. On pointing this out the *O* immediately suggested that "it wasn't the number of images which stretched out the interval but the large number of incipient motor reactions stimulated by them which added to the apparent interval." As far as I can judge from my limited experiments this is the case.

(3) *Privacy.* "The image is private and personal." "It is my image—not a common experience which might be produced by anyone stimulated this way." "I don't mean that I can't have images without self-consciousness, but I don't remember being conscious of self without having images too."

It is perhaps unwise to go too much into detail on this aspect of the image, since it is hardly possible to do so without making more of an excursion into the field of consciousness of self than would be proper here. All that can be safely concluded is that image-consciousness is somewhat more closely connected with self-consciousness than is perception. A similar reservation must be made concerning the motor aspect of images which will be taken up next.

(4) *Voluntariness.* "The image seems especially connected with the will." "The image seems to lead to voluntary action and change, but it is not the willing agent." Nor is the image essentially motor in character, though some selected images may have that quality.

The typical image, at least the type commonly stimulated in acts of perception, is reported by all *Os* to be 'sensory,' 'passive.' "It does seem more free and indeterminate than the percept which seems forced upon me, therefore when the image leads to action it seems free, indeterminate, not automatic, action." "In fact," says this philosophically trained *O*, "most of the characteristic qualities of the image seem to be due to the freedom of the image as compared with the percept. The image is not objective because its stimulation does not necessarily depend directly on impulses from sense organs. It is private because often stimulated by processes not directly perceptual. It is universal because the same image can be stimulated by a large number of different percepts."

(5) *Universality.* "The image, though of a particular house, seems to feel different from the percept of that house, it seems to stand for houses in general, as the word 'house' stands for all possible houses." "Though I had the picture of a particular chair I wasn't thinking of a particular chair but of chairs in general." "The picture (image) is a peg on which I hang the idea.—No; I don't mean that I have ideas apart from images, but there is something more general about images than percepts." Many more descriptions of this important characteristic, which there is reason to believe is not really intrinsic but relational, could be quoted; but we shall have to recur to this aspect later.

It will be noted that the list of properties is very meagre; and, moreover, that there is considerable ground for doubting

whether all or any of these qualities is really intrinsic in the sense that color is an intrinsic quality of some percepts. To the author these qualities are distinctly relational requiring two images; but it is believed that the list covers all qualities of the image that the critical *O*, taught to report only intrinsic qualities and to ignore relations, is able to report. He will probably report less. If *O* is asked to report relations between percept and images, however, (*e.g.* such introspective facts as that the percept leads to, or stimulates, the image) a much wider field for introspection is opened—that concerned with meanings.

It will be convenient first to deal with the relations discoverable by introspection between the image and the percept. Elaborate introspections are hardly necessary to illustrate the main fact in this relation; namely, that a percept often appears to lead to, to suggest, or to stimulate, a particular image. For some percepts the image is normally of its own mode. This will be called intramodal stimulation. For other percepts, in some *O*s at least, the image stimulated is normally of a different mode. This will be called intramodal stimulation. Whether the stimulation is intramodal or transmodal depends on factors which are not entirely known and which will not be discussed. Moreover, the exact mechanism of transmodal stimulation is not entirely clear. In my case the route of transmodal stimulation, so far as I can detect, is often direct. Thus the visual perception (of a cat) stimulates the auditory image 'cat' without an intermediate visual image. In some cases, however, where there is a delay between the visual perception and the auditory image, I do detect what appears to be a visual image, as if the path from visual percept to auditory image was through visual images. This indirect type of transmodal stimulation may be more common than I suppose; but it is not necessary to decide this point here as we shall assume for the purposes of this paper the possibility of both intramodal and extramodal connections from percept to image.

The most striking feature of the stimulation of image by percept is that it is always accompanied by the consciousness of meaning and of a particular type of meaning; namely, that the stimulated image means or stands for a given percept. Thus whenever a visual percept, a spatially localized patch of brown color, stimulates, leads to, or suggests the auditory image 'cow' I am conscious that I perceived a cow. If it does not stimulate such an image, the percept remains without meaning.

"The first effect on consciousness was a gray-blue 'feel' of neutral emotional tone. I was singularly at rest at the beginning of the experiment. There was no muscular shock or other kinaesthetic effect. Almost immediately I was aware of slow kinaesthesis of eye-movements plus perception of a light gray patch across the line of eye-movement. This patch caugbt the eye-movement and carried it away from me giving the full per-

ception of a gray line lying in space. The gray square (the cover of the book) now developed as lying in space, the darker gray (of the table top) being in the background. There seems no emotional value attached, no special strain. As the front edge of the book caught the somewhat wandering focus there was a feeling of slight kinaesthetic shock, *e.g.* the feeling of surprise. I winked. There was slight strain in the throat as if to say 'book.' During this period of slight strain or transitional consciousness I seemed to feel the coming of a meaning, but the full consciousness that the gray figure was a book came only with the auditory vocal image (when I seemed to hear the word 'book' spoken). Before that the gray figure was meaningless; but it did have spatial configuration and was entirely definite."

This is perhaps an exceptional introspection in the simplicity of the processes displayed; but it illustrates first the vague pre-sensation feel from which the percept gradually arises in spatial and existential relation to the parent feel which remains throughout as a background. This percept with its pre-sensational background in a moment of transition, forcasted by pre-sensational strain, stimulates a new bit of experience; and this bit of experience, while not a meaning itself, is accompanied by consciousness of meaning at least when so stimulated.

In the case given the image stimulated is of a mode different from that of the stimulating percepts. It is somewhat more difficult to separate in introspective accounts the image from the percept when they are of the same modes, but there seems to be no essential difference in mechanism.

"It (the percept of an apple) had shape and size and color but no meaning. It was just a localized patch of color. Suddenly the strain pre-sensation broke. There was some kinaesthesis, but I've forgotten what and when I was aware again that it was an apple and had meaning. It was as if I saw that the particular percept was an apple; that is to say that it fitted into the picture which always carries with me the meaning of apple (a frame of green leaves, a bit of blue sky and a suggestion of pink blossoms, all less vivid than looking at an apple but still vivid). Into this picture or scheme the percept seemed to fall as if it belonged there. There were never two apples present. It was just as if seeing the apple brought the picture to me. The 'picture' apple seemed to go with the apple 'seen' but they weren't quite the same in feeling, though they were the same in many ways. It was yellow, for instance, though when I try to think of apples without trying to remember any special apple, I have the same picture in my mind but the apple is red. . . . I'm right about this? The apple I saw was really yellow, wasn't it?"

Other *O*s report the same general stages (a) percept, (b) strained transition, (c) fusion of the percept into a new form or the conscious generation of a new form which may be quite different from the percept and into which it does not fuse, but which always arises out of it.

To one of my *O*s a row of men including the particular man seen always appeared as an image when a particular man was recognized as being a man. In another case a figure was not recognized as that of a man unless it stimulated the image of a particular fearful Chinese gardener, an association which sometimes led to the most grotesque results.

In no case was there meaning without imagery of some sort. The following observation was made. An *O* whose dominant images were auditory had exhibited to him visually a chair, and

at the same time the word 'chair' was clearly spoken to him. Various results were obtained. Occasionally *O* would be totally unaware of one stimulus, *i.e.* he did not perceive it; in other cases it was present as a pre-sensation. More commonly both stimuli were observed. Under such conditions *O* was aware of the customary mixed presentation, and then of the two separate percepts lying side by side without awareness that they were particularly related and without meaning. Then during a transition period, when the attention was fixed on the visual percept and the auditory percept was not noticed, "suddenly the word reappeared as if generated or connected both with the visual chair and with the sound and I was aware that both meant chair."

Though images are necessary for meaning, so also are percepts, at least where the thing meant is regarded as an external object.

It is possible also to arouse images by indirect means not involving the usual percept. The image 'chair' may be aroused by means of other images which merely 'suggest' chair. Or *O* may suddenly and spontaneously 'think about' a chair. Or the image may be forced by asking a question; *e.g.* "What piece of furniture has four legs and a back?" In all such cases the image 'chair' appears, and, except in the second rare case,[3] has a definite meaning in respect to the stimulating source but not the meaning that it would have if stimulated by a percept. Thus in the last case the meaning attached to the imaged chair "is a piece of furniture which has four legs and a back." It does not carry the meaning of an object. Moreover, though both image and percept are necessary that one may mean the other, the mere juxtaposition of the two is not sufficient to arouse this meaning.

Having stimulated the verbal image 'chair' by one of the means just described *O* was shown a chair which he promptly perceived. Though he clearly had the perception-chair and the image-chair, nevertheless the visual percept was without meaning and the image meant "what you sit on" and not the object he saw. It was only after a temporary fluctuation, in which the image faded out and reappeared, "as if stimulated by the percept-picture," that the image 'chair' meant the perceived object.

Numerous experiments have led to the same result. Though the mechanism (presently to be discussed) by which the transition from image to image was brought about was complicated and though in some cases the images stimulated by the percept in turn stimulated other images, thus complicating the observation, nevertheless there was no consciousness that a given image carried the meaning of a given percept when that percept "stimulated," "led to," "forced me to think of," "changed into," "grew into," or "started" the image.

[3]Even in this case it is felt "to have a meaning, but I don't know what."

Now it will be allowed that the comments made are attempts of the *Os* to describe in non-subjective terms the relations which exist during consciousness of meaning, and also that the descriptions may equally well be applied to the physiological phenomena presumably concurrent, *i.e.* the stimulation of neural activity in the lobular area by way of the Rolandic area. When taken separately and all together, however, they suggest a fair definition of what meaning, regarded as content, is. In other words, such an analysis leads to the hypothesis that meaning is nothing more than consciousness of the percept 'A' and the image 'B' together with the conscious fact that one is stimulating the other. It is therefore the stimulation of image by percept that constitutes meaning, even where image and percept are such as not to suggest that one could have any connection with the other.

In one instance the image which carried the meaning 'cow' was a particular ship at sea. A given visual percept was never thought of as a cow unless it led to, or stimulated, this ship image. This ship image, moreover, could only be evoked by stimuli which to the normal person would suggest the 'idea' cow. Thus an attempt to use as a stimulus a picture of a ship which the *O* (a trained artist) had himself drawn as presenting his image failed to stimulate this image at all, stimulating instead the image of quite another ship, which to him conveyed the 'ship' meaning. (Investigation established the fact that there was a strong, though forgotten, association between the two ideas due to an infantile experience.)

More common, but hardly less striking, evidence that there is nothing inherent in percept or image that determines meaning is the case where a percept happens to stimulate not the normal image but some image quite inappropriate to it, as when the picture of a hat stimulates the image 'bat' or 'warship.' Such cases would ordinarily be called errors in perception; but they are in fact the normal analogues of paraphasia, for the perception though 'normal' happens to stimulate a wrong or unusual image. The importance of such cases for our purpose is that, though the image is often totally unlike anything *O* would ordinarily recognize as having a connection with the percept, it has meaning when thus stimulated just as in the more 'normal' instances, the erroneous reference having all the qualities of a meaning.

This concludes our direct introspective treatment of the meaning of percepts, which leads to an extraordinarily simple mechanism for the stimulation of an image-center by a percept-center, a mechanism which is similar in principle to that connecting pre-sensation and percept. Images may be thought of as the activity of a new conscious level, an activity stimulated by percept activity and partly controlling, in its turn, the activity of that lower level, exactly as activity of the percept-arc may be stimulated by activity in the next lower level (that of

the pre-sensation) and in its turn may partly control the activity of that level. A Hughlings Jackson diagram[4] may thus represent the relations between the activity of the image and percept levels just as it does the relations between the percept and pre-sensation levels, the sole difference in the diagram of connections being that, in the case of the percepts and images, the cross-paths from activity of one modality to that of another (indicated on our diagram[5] by oblique lines) are often of high valence while in the connections between pre-sensation and percept high valency is exceptional.

[4]Cf. Fig. 1*a* in the first article of this series, this JOURNAL, 36, 1925, 590.

[5]Cf. Figs. 1*a* and 1*b* in the first article of this series, *loc. cit.*, and also Fig. 1 in the second article, this JOURNAL, 37, 1926, 383.

THE EFFECT OF SUGGESTION ON THE JUDGMENT OF FACIAL EXPRESSION OF EMOTION

By Ellen Jarden and Samuel W. Fernberger
University of Pennsylvania

Some time ago, Buzby[1] reported the results of an experiment with the Boring and Titchener [Piderit] model for demonstrating the facial expression of emotion.[2] He used only six of the faces analysed by Piderit—Anger, Dismayed, Horrified, Disdainful, Disgusted, and Bewildered. Judgments were obtained from 716 *Ss*. No suggestion was given regarding the emotion portrayed. The *Ss* were merely asked to choose the best descriptive term from a list of 18 among which were the 6 correct terms. The results show a wide scatter—in the case of Anger, for example, every one of the 18 descriptive terms was chosen at least once. Buzby's general conclusions, which seem to be clearly justified from his results, are: (1) the total facial expressions of Piderit for Anger and Dismayed do not give a high percentage of correct judgments; (2) those for Horrified, Disdainful, Disgusted, and Bewildered give a high percentage of correct judgments, especially when one combines other descriptive terms which closely resemble the expression represented." The largest number of 'correct' judgments obtained for any one of the figures was 63 percent—*i.e.* for Horrified. Four of the faces gave correct judgments in less than 40 percent of the cases.

The Boring and Titchener model of the Piderit faces has a much more valid demonstrational value than these results would indicate and so it was determined to repeat the Buzby experiment introducing two degrees of suggestion. The same six faces, employed by Buzby, were used throughout the present experiment. The experiment was divided into two parts, each part representing a different degree of suggestion.

In the first part, judgments were obtained from 995 students (men and women, graduate and undergraduate) in the courses in psychology at the University of Pennsylvania. The following instructions were given: "You will be shown successively six faces each of which represents the expression of an emotion. In each case, the correct name of the emotion it is

[1]D. E. Buzby, The interpretation of facial expression, this Journal, 35, 1924, 602-604.

[2]E. G. Boring and E. B. Titchener, A model for the demonstration of facial expression, *ibid.*, 34, 1923, 471-485.

supposed to represent will be given you. You will record (1) whether the face represents the emotion named; and (2) if it is such a representation, you will further record the degree of excellence, using the following terms only 'poor,' 'fair,' and 'good.'" These instructions represent the lower degree of suggestion. The results from this series will hereafter be spoken of as from the 'named' experiments.

Four months later, the same faces were shown to 891 students (almost the same group who had made the judgments before) and with the same instructions. But this time, one of the authors, who has a certain amount of facility for doing this, first assumed the mimic and pantomimic expression of the emotion before the group, analysed the facial expression with regard to brow, eyes, nose and mouth and then, before the group and with verbal explanations, built up the model face to correspond to this analysis. This, we believe, represents the highest degree of suggestion possible under demonstrational conditions. The results from this series will hereafter be spoken of as from the 'analysed' experiments.

Results

The results for both procedures for the total percentage of correct judgments are given in Table I. The results of the present study are compared with those obtained by Buzby, which appear in the first column. In the second column will be found the percentage of correct judgments for the lesser degree of suggestion (named) without regard to the degree of excellence, and in the last column are similar results for the greater degree

Table I

Percent Correct Judgments

EXPRESSION	BUZBY	NAMED	ANALYSED
Anger	2	36	56
Dismayed	6	80	86
Horrified	63	98	95
Disdainful	37	98	98
Disgusted	53	99	99
Bewildered	36	96	96

of suggestion (analysed). These results are shown graphically in Chart I. In this chart, the Buzby results are shown in shaded figures, the 'named' results in white, and the 'analysed' in black.

The percentage of correct judgments for Anger were very low for Buzby (only 2%). Under the suggestion of the name, these increase to 36%, and under the total suggestion of the analysis of the face, they rise to 56%. The results for Dismayed show a somewhat similar course—a marked rise in percentage correct from 6% (Buzby) to 80% when the name is suggested

and a still further increase to 86% correct when the face is analysed. The results for the other four faces (Horrified, Disdainful, Disgusted, and Bewildered) are very similar—increasing for 'named' to 96-99% correct, and showing no further increase with the greater suggestion of analysing the face. There is one slight inversion for Horrified—a reduction from 98% correct for 'named' to 95% correct for 'analysed.' In the cases of Disdainful, Disgusted, and Bewildered, the percentages for the two degrees of suggestion are identical.

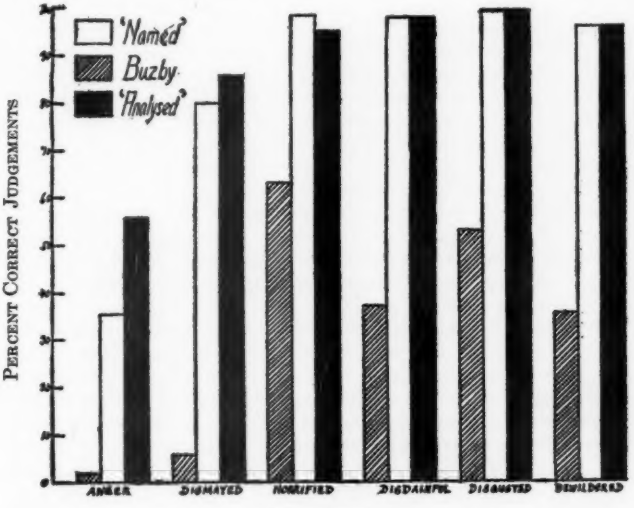

Chart I. Comparison of Buzby's results with those from the 'named' and 'analysed' experiments

The results for Anger and Dismayed seem to indicate that these faces of the Piderit model cannot be brought to a high enough percentage of correct judgments to be adequate for demonstrational purposes, no matter what degree of suggestion is used. For the other four cases, the judgments are so high that we may consider them unanimous choices—and this is equally true for the lower degree of suggestion as for the total analysis of the face.

The results with regard to the degree of excellence are given in Table II. Four columns are given for each of our experimental procedures—the first four contain the percentages of correct judgments for 'named,' and the second group of four

TABLE II

Percent of Judgments with Degree of Excellence

EXPRESSION	NAMED				ANALYSED			
	No	Poor	Fair	Good	No	Poor	Fair	Good
Anger	64	28	7	2	44	35	16	5
Dismayed	20	32	35	13	14	17	41	18
Horrified	2	7	21	69	5	12	23	60
Disdainful	2	7	27	64	2	7	28	64
Disgusted	1	6	22	70	1	7	25	68
Bewildered	4	11	40	45	4	9	38	49

columns for 'analysed.' The percentages for judgments of 'no,' 'poor,' 'fair,' and 'good' are given in each case. There is no means of comparing these results with those obtained by Buzby.

These results are similar, in many respects, to those already discussed for the total percentages of correct judgments. In the cases of Anger and Dismayed, the higher degree of suggestion not only increases the number of correct judgments but it also increases slightly the percentage of judgments with the higher degrees of excellence (fair and good). In the cases of the other four faces, the percentages for the different degrees of excellence show very little change. For all of these four faces more than 80% (and frequently 90%) of the judgments are rated either as 'fair' or 'good'.

A further analysis of these results with regard to sex and also with regard to the degree of psychological sophistication of the *Ss* is shown in Table III. Buzby, in the former experiment, found that "women give a slightly greater percentage of correct judgments than men, and both men and women show a slight decrease in the percentage of correct judgments of facial ex-

TABLE III

Analysis of Judgments with Regard to Sex and Degree of Psychological Sophistication

Degree of Suggestion	Expression	NAÏVE								SOPHISTICATED							
		Men				Women				Men				Women			
		N	P	F	G	N	P	F	G	N	P	F	G	N	P	F	G
NAMED	Anger	62	31	5	2	74	22	4	0	47	37	13	3	66	20	12	3
	Dismayed	15	29	42	14	14	37	36	13	29	29	31	11	33	36	21	10
	Horrified	3	9	19	70	2	5	20	72	2	5	19	74	1	8	27	64
	Disdainful	2	10	25	63	2	2	30	66	2	7	28	63	2	3	30	65
	Disgusted	2	8	24	66	2	1	11	87	2	7	29	62	0	7	23	70
	Bewildered	2	12	41	44	3	7	43	48	7	13	39	41	7	10	34	49
ANALYSED	Anger	45	43	12	1	57	26	12	5	24	39	32	6	31	33	24	12
	Dismayed	9	24	48	20	21	30	36	14	12	38	32	19	14	23	41	21
	Horrified	4	12	25	60	11	12	23	54	4	12	20	64	3	12	21	66
	Disdainful	2	5	25	68	2	7	26	64	0	6	31	63	4	8	33	55
	Disgusted	2	8	31	59	1	3	14	82	1	6	35	58	0	6	23	71
	Bewildered	5	14	40	41	5	8	40	48	6	11	30	54	0	1	37	61

pression with increasing psychological sophistication." The first eight columns of the table—headed 'naïve'—contain the percentages for the men and women respectively in their first course in psychology. The last eight columns—headed 'sophisticated'—percentages of the students who have had at least one year of systematic psychology. A study of this table indicates that the distribution of percentages for both degrees of suggestion are very similar for both sexes and for both degrees of psychological knowledge. This would indicate that suggestion of a greater or lesser degree has the same effect upon either sex and upon the psychologically sophisticated as well as the psychologically naïve.

These results would seem to have a considerable bearing on the technique for the use of the Piderit model for demonstration purposes. It seems obvious that the emotion which each of the faces is supposed to represent should be named early in each demonstration; and that, unless this condition is complied with, the face will not be correctly recognized by a sufficiently large number of the group. Once the face has been named, the analysis of the face, which is the thing of psychological importance, will apparently not increase the number in the group who will recognize the total emotional expression.

These results seem to have a certain application to the psychology of pictorial advertizing. From a consideration of the present data and those of Buzby's experiment, it would seem that the printing of a picture—supposed to represent an emotional expression—will not be recognized by any large majority of the readers unless the emotion is named or otherwise indicated early in the 'copy.'

Conclusions

(1) Suggestion, such as the naming of the emotion and the analysis of the face, increases to a considerable degree the percentage of correct recognitions of the faces of the Piderit model.

(2) In the cases of the faces for Anger and Dismay—which do not give a high percentage of recognitions when no suggestion is given—the amount of increase is progressive with increasing degrees of suggestion.

(3) The angry face does not seem, even under the conditions of the greatest degree of suggestion possible, to give a large enough percentage of correct recognitions, to make its use for demonstration purposes advisable. The results for Dismay, although much better than those for Anger, leave much to be desired.

(4) For those faces such as Horrified, Disdainful, Disgusted, and Bewildered, which give a relatively high percentage of correct recognitions when no suggestion is given, the suggestion of giving the name is sufficient to bring these to almost

unanimous recognition. The further suggestion given by the analysis of the face does not improve the percentages of correct recognitions.

(5) The conclusions indicated above are equally true for either sex and for all degrees of psychological naïveté or sophistication.

(6) Certain technique for the use of the Piderit model for demonstration purposes and certain applications to the psychology of pictorial advertizing are indicated.

SYNAESTHESIS: 'PRESSURY' COLD

By Karl M. Dallenbach, Cornell University

Last semester during the routine experiments, in the qualitative laboratory, on the temperature spots, one of the students (Miss Alexandra Hobart) made the very interesting observation that "some of the cold spots are felt in my teeth;" that is, besides the primary cold sensations localized upon the forearm where the stimuli were applied, she experienced at times secondary sensations of indefinite character localized in and around the teeth and cheeks.

Questions regarding these experiences brought out that we were probably dealing with a type of synaesthesis as yet undescribed, *i.e.* 'pressury' cold;[1] we learned also, that *O*'s mother (Mrs. M. F. McClure Hobart), a student in this laboratory 27 years ago, had a complicated form of synaesthesis—studied and described in detail by Whipple.[2] Because we apparently had here a type of synaesthesis that was new, and one that might also cast some light upon the hereditary theory of synaesthesis, it seemed worth while to make a further study of this case.

I. Description of Experiments and Results

Experiments. The routine laboratory experiments for cold were repeated.[3]

Experiments were first performed upon the area that gave anomalous results—the radial side of the left forearm, 16.5 cm. above the carpal folds. When the casual observations of the class experiments were verified, the experiments were extended to an area located symmetrically upon the right forearm; to areas on the external parts of the right and left upper arms, 20 cm. from the elbow; and to areas on the right and left legs, 8 cm. above the patella. These parts of the body were shaved, so as to avoid the possible distractions of tickle from the hair, and a map, 2 cm. sq., bearing cross-section lines 2 mm. apart, was stamped on the skin. The areas were explored with temperature cylinders[4] cooled by cracked ice. The

[1]The literature on synaesthesis is very extensive. Cf. the reviews and bibliographies of F. Suarez de Mendoza, *L'Audition colorée*, 1890, 1-139; W. O. Krohn, Pseudo-chromaesthesia, or the association of color with words, letters and sounds, this Journal, 5, 1893, 20-41; F. Wehofer, 'Farbenhören' (chromatische Phonopsien) bei Musik, *Zsch. f. angew. Psychol.*, 7, 1913, 1-54; and R. H. Wheeler, The synaesthesia of a blind subject, *Univ. of Oregon Pub.*, 1, 1920, No. 5, 1-61.

[2]G. M. Whipple, Two cases of synaesthesia, this Journal, 11, 1900, 377-404.

[3]Miss Hobart's laboratory partner, Miss Harriet W. Leaf, acted as *E* in all the experiments reported here.

[4]For a description of the apparatus see E. B. Titchener, *Experimental Psychology, Student's Qualitative Manual*, 1900, 54.

cylinders came to a point 1 mm. in diam. Stimulation was punctiform, and proceeded at the rate of one a second. Every square millimeter of the areas was worked over.

O was blindfolded and instructed to give a complete report of her experience, but to pay particular attention to the experiences of cold and to the concomitant experiences in the teeth and cheeks. When either an 'ordinary' cold or a 'toothy' cold spot—for thus did *O* designate the spots that gave the concomitant experiences—was discovered its position was marked upon a duplicate map by dots and crosses respectively. When the 'toothy' spots were reported, *O* attempted, by pointing and by verbal description, to localize the concomitant experience. When an area had thus been worked over and mapped, *O* gave a description of the concomitant experiences. Twenty four hours later, before the map had faded from *O*'s arm, the area was worked over in the same manner and a second map obtained.

Results. The results of these experiments corroborate the casual observation of the laboratory exercises. *O*'s cold spots were of two kinds: (1) those that gave only the "bright, flashing, lancing" cold experiences with which we are familiar; and (2) those that yielded additional experiences, localized in the teeth and cheeks. The average number of these two kinds of spots per sq. cm. found in the areas investigated is shown in Table I. No great significance is to be attached to the absolute frequencies, for the procedure was rough, and both *O* and *E* were relatively inexperienced. The significance of the data, however, lies in the fact that 'toothy' spots were discovered in both mappings, in all six of the areas explored. The relative frequency of the two kinds of spots varied greatly from area to area; on an average, however, there was 1 'toothy' to 2 'ordinary' spots. A surprisingly large number of spots were re-obtained in the second mapping. The data in Table I show that of the total number discovered 69% of the 'ordinary' and 81% of the 'toothy' colds were verified.

TABLE I

Showing the number of 'cold' and 'toothy' spots obtained per sq. cm. from the right and left forearms, upper arms, and legs; and the number and percent verified at the second mapping

AREA	Obtained in first mapping		Verified in second mapping		Percent of verification	
	cold	toothy	cold	toothy	cold	toothy
left forearm	7½	3½	5½	2½	73	71
right forearm	4½	5	3	4¼	66	85
left upper arm	5½	1¼	4	1¼	72	100
right upper arm	2¾	½	1½	¼	54	50
left leg	4¼	½	3	¼	71	50
right leg	2¾	1½	1¾	1½	58	100
TOTAL	27¼	12¼	18¾	10	69	81

In the first series of experiments—those upon the left forearm—all the secondary experiences were localized to the left of

the mesial plane. The experiments on the right arm and leg were undertaken to see whether this bilateral relation would persist. It did without a single exception; the concomitant experiences were always localized on the side of the body to which the stimulus was applied.

Within this limit, the secondary experiences were variously described and localized by *O*. At times the experiences were reported as 'cold' and localized in a specific tooth. Examples are:

"Cold in right upper front tooth" (right forearm stimulated);
"Cold sensation in tooth on right side lower row" (right upper arm).

For the most part, however, they were described as "pressury feels" and indefinitely localized in the teeth and cheeks, and occasionally in the eyes, ears, temples, head and neck. Examples are:

"That spot gave a reaction in the teeth. Began with a feeling in front tooth, spread to the left and disappeared" (left forearm);
"With that spot I experienced a flash in the cheek and neck" (left forearm);
"Vague sensation in left ear" (left upper arm);
"Pressury feel around left ear" (left leg);
"Pressury flash besides the left eye" (left leg);
"Pressury feel in right temple" (right leg);
"Ache in head much like that caused by eating ice cream" (right leg);
"Sensation in right eye ball" (right leg).

Movement in the secondary experiences was frequently reported. Examples are:

"Began with a feeling in front upper tooth, spread to the left and disappeared" (left forearm);
"The pressury experience begins most often in front teeth above the enamel line, that is it starts there and spreads out to one side or the other depending upon the side stimulated. It is not, however, always definitely localized" (introspection given at conclusion of the experiments).

The secondary experiences were usually concomitant with the primary. On a few occasions, however, a temporal disjunction was reported. Examples are:

"After a very cold spot, I had a sensation in the left front tooth" (left leg);
"Developed into tooth sensation. Seemed as though the stimulus had been held on the spot" (right forearm).

In order to determine whether cold was the necessary stimulus, an attempt was made to stimulate a few of the 'toothy' spots by pressure, but to no avail. Cold seemed to be the only adequate stimulus.

The examination of this case reveals that we have here phenomena that are synaesthetic in type. They are intermediate in complexity between colored hearing, colored tastes, odors, etc.—phenomena that involve the higher senses; and the 'thrills' and 'goose-flesh' occasioned by the hearing of the rasp

of a file, the scrape of the finger nails, or the squeak of chalk upon the blackboard,[5] etc.—experiences which involve the lower senses and which are more or less common to all mankind.[6]

II. Discussion

The importance of this case lies not only in the fact that we have here a new type of synaesthesis, but also in the fact that it may throw some light upon the hereditary hypothesis first advanced by Cornaz[7] in 1848, and since then both denied and re-affirmed by many investigators. The evidence against the hereditary theory of synaesthesis and in favor of the associative theory lies chiefly in the fact that case histories and questionary results do not show that the trait has been transmitted from one generation to the next. But questionaries are notoriously inaccurate, and case histories are adequate only where the parents or children of the synaesthetic subjects are personally interviewed and examined. To ask a synaesthetic subject about the peculiarities of his parents' or children's experiences is the questionary over again, but at one step removed, with the chance of error and inaccuracy greatly increased.

We know the facts about our *O*. Though her mother has synaesthetic images with noises, tones, tastes, and smells, and also a few personifications, our *O* has, besides the pressury colds just discovered, no other form of synaesthesis. Had she been asked previously to this experiment if she had any synaesthetic images, she would have replied in the negative. If our *O*'s mother had been asked whether her daughter experienced any such phenomena her reply would likewise have been in the negative. A questionary or the ordinary case history would have failed to reveal the facts in this case.

An hereditary theory does not necessarily imply that the same kind of synaesthetic images should be experienced by parent and offspring. Nothing more is hypothesized than that the offspring—or some of them, for synaesthesis, like color blindness, may prove to be sex linked—inherit a peculiarity of nervous

[5]The ice-cream pain variously localized in the head, though of extremely common experience, are not included here, for it is very doubtful whether they are of synaesthetic origin. Until more is known about them they must be regarded simply as referred pains (Cf. E. G. Boring, Sensations of the alimentary canal, this Journal, 26, 1915, 25, 35 ff.) originating in the stimulation of the esophageal and gastric branches of the vagus, or due to the faulty localizations of the sensations aroused in the esophagus.

[6]Whether they are common to all mankind is undetermined, since no reports, so far as I have been able to discover, have been made concerning the experiences of shrill sounds by aborigines.

[7]C. A. Cornaz, *Des abnormités congénitales de jeux et de leurs annexes*, 1848.

constitution—unusual anastomoses or abnormal crossing and uniting of afferent fibers—which will be evinced in some one or more of the many types of synaesthesis.[8] There is no *a priori* reason, however, why the peculiarity of nervous structure should, in different generations and different individuals, be equal in extent or identical in location. One generation may show one type of synaesthesis, the next another. Or, as in the present instance, the mother may have photisms with tones, noises, smells, and tastes, and the daughter 'pressury' colds; yet a common hereditary trait may be responsible for both.

In giving the case history of *O*'s mother, Whipple reports that, "no trace of colored hearing can be found in the other members of the family."[9] Whether colored hearing specifically existed is unimportant, what is important is whether either of the parents had synaesthetic images of any kind. Furthermore, Whipple does not tell us whether he based his statement upon an examination of his subject's family. The presumption is that he did not, that he relied, as so many investigators before and since him have done, upon his subject's knowledge and report. This, particularly when it is negative, has but little value; one individual's knowledge of another's—even a parent's—mental peculiarities may be practically nil. A parent may have a pronounced case of synaesthesis; yet the children may know nothing of it. Even questions put to the individual concerned may fail to bring out the fact, either because the questions are too specific and do not cover the particular variety involved, or, as in the case of our *O*, because the individual himself may be totally unaware that he has synaesthetic images. It is only when the peculiar conditions necessary for their arousal are present that they may make themselves known.

The fact that synaesthetic images appear suddenly and are sometimes unnoticed until comparatively late in life, as in the case of our *O*, is brought in evidence against the hereditary theory. Whipple reports that *O*'s mother at the age of 16 suddenly experienced colored hearing, and remarks that "the hypothesis of heredity seems to be refuted by the fact that the first instance of the phenomenon was quite sudden and hallucinatory in character, and of late origin."[10] Suddenness and lateness of appearance is, however, no argument either for the associative theory or against the hereditary. They are merely facts that need explaining, and indeed when it comes to that they are more difficult to explain by the associative theory than by the hereditary. As a *post hoc* explanation, the associative theory may be

[8] Wheeler lists 34 varieties, *op. cit.*, 32 f.

[9] Whipple, *op. cit.*, 378.

[10] *Ibid.*

stretched to explain the various phenomena experienced afterwards, but it is totally unable to account for the first appearance. Both of these points, however, may be accounted for upon the hereditary hypothesis by assuming that the inherited neurological condition was there, but that the essential objective conditions for the appearance of synaesthetic images were not previously present. This seems certainly to be the explanation of the suddenness and lateness of the appearance of the 'pressury' colds experienced by our *O*, for until she came to the laboratory experiments on cold the objective conditions had never been so favorable. It was only when the cold spots were stimulated separately and she was free from distractions, in the objective attitude, and set for a description of experience, that she suddenly became aware of the synaesthetic images.

Now that these images have been focalized, they may of course be experienced under other and less favorable conditions; but this does not mean that the first sudden experience established an associative mechanism which is responsible for their later appearance. What factors, one might well ask, were responsible for the association in the first instance; and, in view of our results, why was the associative mechanism established only in the case of some of the cold spots—about one-third of them—and not for others.

Suddenness and lateness of occurrence, moreover, do not necessarily imply that the concomitant phenomena had never before been experienced. Doubtlessly, they had been previously experienced; but since they had no meaning and played no functional part in mental life, the subject ignored them, brushed them aside, without ever becoming fully aware of them. We do not for a moment believe that our *O* never before had 'pressury' colds, but only that never before had she become distinctly aware of the pressure component. The case is similar with the positive and negative after-images of vision and with many other mental phenomena that come to most students of psychology as 'new,' as something never before experienced. The lecture demonstration is not the first time that these phenomena have been aroused; it is merely the first time that they have been consciously present. A single demonstration is all that is needed to set the students observing their after-images under other conditions. Knowledge that such phenomena occur is all that is necessary. The case, we feel, is similar with synaesthesis. A particular subject may be unaware that he has synaesthetic images until conditions arise in which they are so clear, so intense, so insistent that he is forced to attend to them. They will of course be described as coming suddenly, and—if this collocation of conditions occurs comparatively late in life—as late. Once attended to, the synaesthetic images may recur in

experience; but, as with the after-images, this merely shows the importance of 'knowledge about;' it is no argument against the hereditary hypothesis.

Summary

The findings of this study may be summarized as follows:

(1) A type of synaesthesis—'pressury' cold—hitherto undescribed has been reported.

(2) *O*'s cold spots were of two kinds: the 'ordinary' spots that gave the usual experiences, and 'toothy' spots that gave also secondary experiences localized in and around the teeth and cheeks.

(3) The 'toothy' cold-spots occur on all the parts of the body experimented upon—the right and left forearms, upper arms, and legs—though with varying frequency.

(4) The secondary experiences aroused by stimulation of the 'toothy' spots were always localized upon the side of the body stimulated.

(5) A second mapping, made 24 hours after the first, verified 69% of the 'ordinary' colds and 81% of the 'toothy' colds.

(6) The secondary experiences were described as light, transient, and pressury in quality.

(7) The results of this case, in view of the fact that *O* has a synaesthetic inheritance, appear as evidence of the hereditary theory of synaesthesis.

ON THE NUMBER OF ARTICLES OF PSYCHOLOGICAL INTEREST PUBLISHED IN THE DIFFERENT LANGUAGES (1916-1925)

By SAMUEL W. FERNBERGER, University of Pennsylvania

Ten years ago, the author published an article,[1] under this same title, in which he attempted to demonstrate certain national trends in psychology by an analysis of the languages in which the titles were written and which were listed in the *Psychological Index*. The author recognizes that such an analysis is not to be construed as indicating the *value* of the contributions of the different countries. Nevertheless, it would seem to be an index of the professional interest in psychology taken by the different countries. If for no other reason, the study seemed justified by indicating the following conclusion: "Perhaps the most striking point brought out by this study is the extreme necessity for the student of psychology—no matter of what nationality he may be—to have a facile and critical reading knowledge of both German and English." It has seemed worth while to extend this study to a consideration of the psychological product of the last ten years. This period includes the end of the World War and it is of interest to discover if this conflict has had any effect upon psychology in the different countries.

An analysis of the data in the former article, covering a period of 22 years (1894-1915) showed that up to 1903 the English, French and German speaking countries were each publishing approximately the same number of articles each year. The year 1903 seems to be a critical one in which all of the curves are more or less affected. The curve for French titles declines steadily from then on to 1915. The curve for English titles has a tendency to rise steadily and slowly but irregularly. The curve for German titles rises very rapidly in 1903, with a sudden increase of more than 1,000 titles in the year and this high position is reasonably maintained until the war period. Then in 1914 and 1915 there are marked decreases in the German curve so that, in this latter year, the number of English titles is greater than the German. Throughout this period the number of Italian titles is much smaller than any of the other three languages and this curve maintains at approximately the same level. The number of titles published in languages other than German, English, French and Italian is so inconsiderable that they may be left out of consideration.

[1] This JOURNAL, 28, 1917, 141-150.

TABLE I
Number of Titles in the Different Languages

Date	English	German	French	Italian	All Others	Total
1916	1682	526	126	83	2	2419
1917	2153	88	233	117	67	2658
1918	1126	218	67	157	18	1585
1919	1464	331	273	252	21	2341
1920	1769	765	195	202	39	2970
1921	1544	432	293	141	20	2430
1922	1837	1701	341	170	28	4077
1923	2015	1011	326	270	33	3655
1924	1772	1459	290	335	45	3901
1925	2082	1214	380	506	28	4210

The results for the present ten-year period (1916-1925) are given in Table I. This table contains the actual number of titles published in English, German, French and Italian and recorded in the *Psychological Index* each year. Titles of all other languages (mostly Scandanavian) are grouped together in a fifth category. The total number of titles listed in the *Index* are also given.

These results are represented in curves in the accompanying figure. An analysis of these curves indicates that there have been more English titles published each year, during this ten-year period, than in any other language. During the American entry in the war (1918) the curve drops very considerably, but from then on the rise has been slow but steady, very similar to the rise in the curve for English titles before the war.

The curve for the number of German titles continues to drop markedly throughout the war period—the low mark being reached in 1917 with only 88 titles recorded. From then on the increase has been considerable. From 1922 on, this curve seems to have hit a dead level—not so high as in the pre-war period. The peak for 1922 is anomolous because in this year the *Index* included a number of back titles actually published during the war period. So the German curve actually should not be quite so low at the trough or quite so high at the peak.

The French curve was dropping markedly before the war period and this decrease in the number of titles was accentuated during the war. In 1919 there was a marked increase and a dead level has been maintained from then on to 1925. This level is considerably lower than that maintained by the French speaking peoples prior to 1910.

It has been considered worth while to include the Italian curve in the present study rather than to throw these results into the general category of 'all others.' The number of Italian

titles was also decreased during the war period. Then a rather low dead level was maintained until 1921. Since that date there has been a steady and marked increasee in the number of Italian titles. This increase has been so great that in the last two years (1924 and 1925) there have been more Italian titles than French.

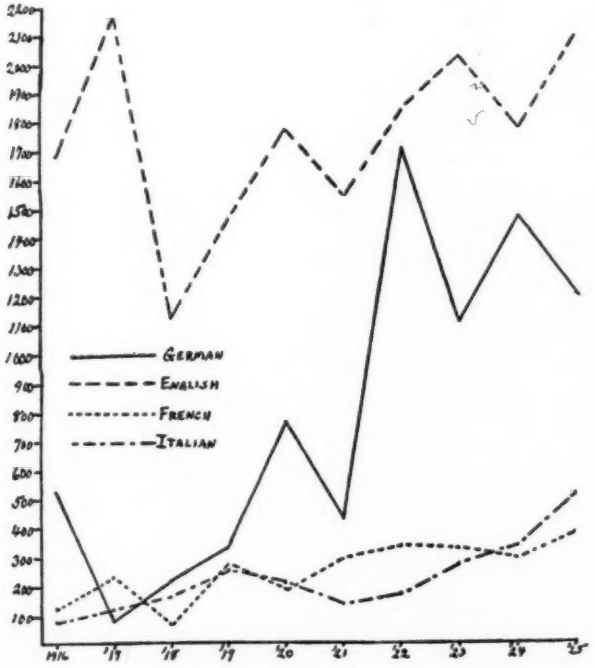

FIG. 1

Treating the Italian results separately leaves such a small number of titles in other languages (mostly Scandanavian) that it has not seemed worth while to include the curve for these. In a figure of this size, such a curve would be very close to the abcissa and would be practically parallel to it with only small fluctuations.

These data have also been thrown into the form of percentages of the titles published in the different languages. These percentages are given in Table II. The percentages of

TABLE II
Percentage of Titles in the Different Languages

Date	English	German	French	Italian	All Others
1916	69.5	21.8	5.0	3.4	0.3
1917	81.0	3.3	8.8	4.4	2.5
1918	71.0	13.7	4.2	9.9	1.2
1919	62.5	14.1	11.7	10.8	0.9
1920	59.6	25.8	6.6	6.8	1.2
1921	63.6	17.7	12.1	5.8	0.8
1922	45.3	41.7	8.7	4.2	0.7
1923	55.2	27.7	8.9	7.2	0.9
1924	45.4	37.4	7.4	8.6	1.2
1925	49.5	28.9	9.0	12.0	0.7

titles in languages other than English, German, French and Italian are so small that they may be disregarded. Only four times do either the French or Italian percentages exceed 10 percent. (French 1919 and 1921; Italian 1919 and 1925). When one combines the English and German percentages the range is from 76.6 percent. in 1919 (the high years for French and Italian) to 91.3 percent. in 1916. In eight of the ten years covered, the combined percentages of English and German titles are more than 80 percent. of the total.

From these data it would seem that the research psychologist of today should be something of a linguist. A reading knowledge of English and German is essential if he is to read the literature in any field. If he is to adequately cover *all* of the field of psychology—and who has the time to do it!—he should have a reading knowledge of French and Italian as well. If he is working in any special field, he must still read English and German. For some fields he is now better off with an additional knowledge of French—in others, with an additional knowledge of Italian. All four languages are desirable, however.

I cannot do better than to restate, as a general conclusion, what I said in the paper ten years ago. The present data, if anything, increase the validity of that final statement. "Perhaps the most striking point brought out by this study is the extreme necessity for the student of psychology—no matter of what nationality he may be—to have a facile and critical reading knowledge of both German and English."

DEVELOPMENT OF LABORATORY EQUIPMENT IN PSYCHOLOGY IN THE UNITED STATES

By C. A. Ruckmick, University of Iowa

With recent interest in the applications of psychology in a great variety of contiguous fields the question has frequently arisen, how does 'pure' psychology fare today in the amount of emphasis placed upon it among psychologists at large; how has it prospered in the progress of its natural growth? We might attempt to answer the question on the basis of several lines of investigation, but the most fruitful hope of ascertaining a natural growth seems to lie in obtaining information concerning the steady development of laboratory equipment in this country. Obviously apparatus does not make a science but in the same sense that the material aspects of a community reflect its prosperity, so the continued demand for apparatus is a fair indicator of the health and vigor of the experimental method.

For a time at least superficial evidence, like the amount of space devoted to advertisement of new 'test-forms' and the amount of published investigations based upon mental tests, gave many of us the impression that paper test materials were supplanting the usual technical apparatus in our laboratories. A detailed study reveals, however, that such is not the case.

While much emphasis is placed upon applied psychology we find, however, that the situation in pure psychology is about the same as it is in physics. The great development of engineering in its several branches, exploited as it is in many of the technical and popular journals has not retarded the quiet growth of pure physics. On the contrary it has stimulated additional research along lines of greatest pressure in the world of application. Something similar seems to be happening just now in psychology where the deficiency of the mental test program on the side of interest, 'drive,' and moral ideals is one of the dynamic forces toward a better understanding of the emotions and of the motivated self from the point of view of pure psychology.

With this incentive a study of the early history of laboratory equipment in this country was undertaken and a survey was made of its present status expressed in financial value, both on the side of a complete inventory and of an annual budget. In a sense this supplements a study made over a decade ago.[1]

[1] C. A. Ruckmich, The history and status of psychology in the United States, this Journal, 23, 1912, 517-531.

If we go back to about 1890, seven years after the establishment of the first psychological laboratory at Johns Hopkins University, we find many of the laboratories in this country in their infancy. The manufacturers of apparatus began making psychological materials about 1892 when a Philadelphia firm, Elmer Willyoung & Company, made up apparatus according to designs furnished by Scripture of Yale. A year afterwards the Garden City Model Company of Chicago produced instruments from suggestions made by Jastrow of Wisconsin. Other manufacturers of that day include J. D. Brown, of Camden, New Jersey, designing apparatus for Cattell and Witmer, and A. Pfeifer of Baltimore who coöperated with G. Stanley Hall. These arrangements seem to be patterned after the European custom where we find the Cambridge Scientific Instrument Company making anthropometrical apparatus for Galton; Diederichs of Göttingen coöperating with Schumann; Krille of Leipzig, with Wundt; and Elbs of Freiburg, with Münsterberg. Besides these, Baltzar of Leipzig, Koenig of Paris, Kohl of Chemnitz, and Peyer, Fararger & Company of Neuchâtel, were designing special types of apparatus like kymographs, tuning-forks, tonometers and Hipp chronoscopes.

At this time the isolated makers in this country fared very poorly and the importations from Europe increased in volume. Presently, about 1899, succeeding the Chicago Laboratory Supply and Scale Company, which in turn had become successor to the W. A. Olmsted Scientific Company, Stoelting[2] of Chicago began to devote more and more of his time and resources to the manufacturing of psychological apparatus. He had the support of some of the more prominent psychologists of that decade, like Jastrow, Sanford, Scripture, Seashore, and Titchener, but psychological apparatus even then did not give profitable returns until the publication of Whipple's *Manual of Mental and Physical Tests*, which created a demand for many distinct pieces of apparatus in addition to test material. With the increased interest in psychology during and after the war, the firm confined its efforts to psychological apparatus, and is now selling material valued at about $500,000 per year. This covers, of course, not only a domestic demand, but exportation to foreign countries, especially to the Orient. The Stoelting Co. has equipped the psychological laboratories of institutions in Australia, Canada, China, England, India, Japan, Panama, Peru, Portugal, New Zealand and South Africa. Orders for apparatus aggregating over thirty-five hundred dollars have recently been filled for the Universities of Peking and Mexico, Maharaja College, at Mysore, India, and Brahmavidvashrama of Adyar, Madras, South India. This firm is now the most important producer of psychological apparatus in this country and the only one specializing in the field on an extensive basis in the country. Of course, this does not convey a complete picture of the development of psychological apparatus throughout the world because large quantities of instruments are being exported from Germany to many nations everywhere.[3]

But our task is confined to the development of laboratory apparatus in this country. Our American laboratories began their consistent growth about 1890. This is not of course the date of their actual beginning which was several years earlier and is indeed somewhat in dispute;[4] but it does mark the start of a continuous and generally wide-spread development of laboratories in our colleges and universities.

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[2] I wish here to express my appreciation to the Stoelting Co. for furnishing me with most of the information contained in this paragraph.

[3] M. Bentley, Experimental psychology in the Orient, this JOURNAL, 37, 1926, 154.

[4] C. A. Ruckmich, *op. cit.*, 519 ff.

The estimated total value of psychological materials in this country in 1893 was only $30,000.[5] About 10 years later Miner estimated that 25 of the largest colleges in the country averaged about $4,000 in total equipment, equivalent to a sum of over $100,000 invested in psychological apparatus. The most expensively equipped at that time were Clark, Columbia, Cornell, Harvard, Pennsylvania, and Yale with a valuation of about $10,000.[6] The earliest accounts of laboratory equipment on which any financial estimate can be based are those of Harvard and Cornell. In 1893, Harvard issued an inventory of the laboratory as a part of the exhibit at the World's Fair. Münsterberg says of it at the time, "the outfit is even now the most nearly complete that is anywhere at the disposal of students in psychology,"[7] and adds as a further reason for publishing the account that "experimental psychology is too often confused with experiments upon the brain by vivisection, with hypnotism, and even with spiritism. . . . This account of our experiments may also serve to correct the error which is so prevalent that experimental psychology is the study of sensations and simple reaction times." He then proceeds to list the studies that are being undertaken under the headings of association, attention, memory, judgment, space, time, the feelings, and the will. In the catalogue we find a total of 240 pieces valued at a little over $4,000. A glance at the table summarized from this catalogue will show the distribution (Chart 1). In 1900 the Cornell laboratories published a similar inventory[8] (Chart 2). An outline of the laboratory and its requirements was given by Titchener two years before. There are 375 items listed at a value approximately equal to $6500 when foreign values are converted into American denominations. About this time Titchener and Cattell[9] dis-

CHART 1

HARVARD PSYCHOLOGICAL LABORATORY—1893

(Nos. in parentheses refer to number of items listed)

I. Objects for anatomical and physiological demonstration		$ 682.10
A. Brain		
a. Human (21)	$ 383.50	
b. Vertebrate (5)	50.00	
B. Sense-organs and nerves		
a. Anatomical (11)	183.60	
b. Physiological (5)	65.00	
II. For study of sensation		$1494.00
A. Hearing (24)	$ 780.00	
B. Sight (36)	578.50	
C. Dermal and muscular Ss. (11)	135.50	
III. For higher mental processes		$1696.20
A. Time measurement (33)	$1109.20	
B. Perception, space and time (25)	261.00	
C. Association, attention, emotion (20)	326.00	
IV. Technical outfit		$ 703.05
A. Optical (14)	$ 315.00	
B. Electrical (15)	227.30	
C. Surgical (20)	160.75	
TOTAL (240)		$4249.35

[5] W. O. Krohn, Facilities in experimental psychology in the colleges of the United States, *Report U. S. Commissioner of Educ.*, 1890-91, 1139.

[6] J. B. Miner, The changing attitude of American universities toward psychology, *Science*, 20, 1904, 303.

[7] H. Münsterberg, *Psychological laboratory of Harvard University*, 1893.

[8] E. B. Titchener, The equipment of a psychological laboratory, this JOURNAL, 11, 1900, 251-265.

[9] J. McK. Cattell, The psychological laboratory, *Psychol. Rev.*, 5, 1898, 655-658.

CHART 2

CORNELL PSYCHOLOGICAL LABORATORY—1900

(Nos. in parentheses refer to number of items listed.)

A. Physiological and psychological acoustics (67)		$1979.75
I. Anatomy and physiology of ear	$ 67.50	
II. Auditory sensation: intensity	157.75	
III. Auditory sensation: quality and clang tint.	1549.95	
IV. Auditory perception, memory, etc	204.55	
V. Charts		
B. Physiological and psychological optics (119)		$1386.90
I. Anatomy and physiology of the eye	$ 37.50	
II. Visual sensation: intensity	106.70	
III. Visual sensation: quality	307.40	
IV. Visual sensation: contrast, etc	138.20	
V. Visual perception	219.75	
VI. Visual association, memory	78.00	
VII. General optical apparatus	499.35	
C. Haptics and Organic Sensation (50)		$ 469.45
I. Haptical, etc. sensations: intensity	$ 74.50	
II. Haptical, etc. sensations: quality	193.45	
III. Haptical, etc. perception	201.50	
D. Taste and smell (17)		$ 105.00
I. Sensations: intensity and quality	$ 104.00	
II. Anatomy and physiology: charts	1.00	
E. Affective processes (36)		$1234.70
I. Special instruments	$ 277.00	
II. Models	14.85	
III. General recording instruments	942.85	
F. Action (18)		$ 624.00
G. Attention (5)		65.50
H. General supplies (63)		626.55
I. Electrical outfit	$ 275.80	
II. Mechanical and chemical	194.00	
III. Physiological and anatomical	108.50	
IV. Diagrams	48.25	
TOTAL (375 items)		$6491.85

cussed in published articles the requirements of a psychology laboratory and specified sums which today at our increased scale of prices seem somewhat fantastic. Cattell considered the Cornell laboratory particularly fortunate in receiving a grant of $2000 for its establishment in 1891 with an annual support of $600, but criticised Titchener's "estimate of $300 for the current expenses" as "rather extravagant," to which Titchener replied[10] "all that my paper says is that it is unsafe to begin the working year with a less sum than $300 in hand." Concurrently articles appeared questioning the use of what was then known as the "new psychology," or the "experimental method" in the teaching of elementary psychology, and the necessity of laboratory equipment was seriously doubted, curiously enough, at an institution where now many researches are annually turned out.

Let us now glance at the situation some twenty-five years later. From the modest equipment of a $2000 laboratory with an annual budget of $600, we turn to a maximum laboratory evaluation of $32,000 with an annual budget as high as $3500. Of course, it may be said that our universities have grown in the meantime, but our charts show no intimate relationship between student enrollment and financial support for the

[10] E. B. Titchener, A psychological laboratory, *Mind*, 7, 1898, 311-331.

psychological laboratory (Charts 3, 4). An institution which has a little over 5000 students comes to the top of the list as far as pure psychology is concerned. Whereas, an institution with the largest enrollment in our group has but one-third of this amount as the present estimated value of its laboratory. The chart (Chart 5) shows roughly the distribution of the values for total laboratory equipment as estimated by responsible individuals at each institution. The total equipment for all of the 32 in

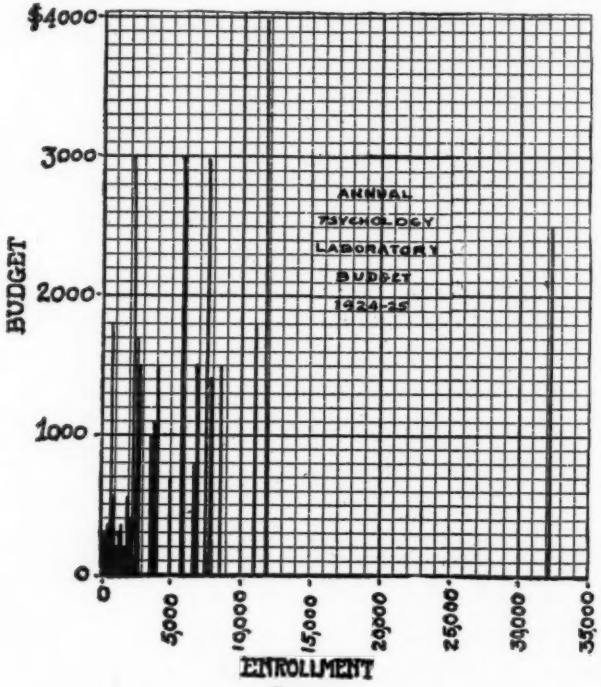

CHART 3

stitutions is about $300,000. The total annual budget of these institutions for apparatus is $35,000.

The questionary which was sent out covered the following items: (1a) the present total evaluation of laboratory apparatus; (1b) of test material; (2) present annual budget; (3) any special funds like endowments, received for these purposes. Information was also requested concerning the employment of a mechanician, and the extent to which he constructed new apparatus. Out of 60 requests to fill out blanks sent to the most representative institutions of the country, 32 replies were received. Since some of the larger institutions either failed to reply or explained the reasons for not making an estimate, the actual returns could probably be duplicated

for the laboratories not represented in the report. In addition, there must be altogether a large amount of material in our normal schools, smaller colleges and universities to which no request was sent. Therefore, on this basis a conservative guess would be to quadruple the amount represented in the report, so that probably our psychological laboratories over the country contain apparatus worth in the neighborhood of a million dollars.

The average annual budget is in the neighborhood of one thousand dollars, ranging from $50 to $3500. This is about twice the average amount appropriated according to a study made of 21 representative institutions in 1911.[11] The latter sum included other items, however, like books and

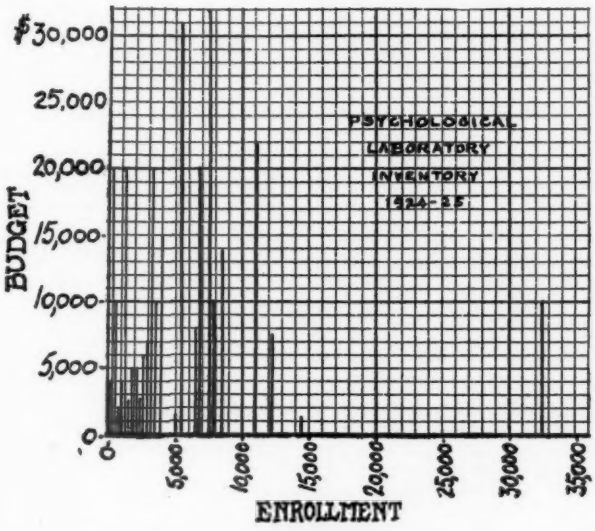

CHART 4

charts, which were not included in the present account, and is therefore not strictly comparable with our present figure. But it is likely that the ratio would be still further increased if the library expenses were deducted from the budgets of 1911.

The report is probably weakest in its attempt to compare the support given to applied psychology in the form of test material as compared with the parallel branch of pure psychology. The reason for this lies in the great variety of affiliations that exist between these two main branches of psychology as represented in departmental and inter-departmental relationships at various academic institutions. In many cases the individual who replied to the questionary could not at all speak for the work in applied psychology, but there is nevertheless some significance in the distribution of funds, as specified in the columns of our table, which reveals a remarkably definite emphasis on pure psychology.

[11] C. A. Ruckmich, *op. cit.*, 528.

CHART 5

Institutions (32) Enrollment	Inventory: Apparatus	Inventory: Test materials	Annual Budget: Apparatus	Annual Budget: Test materials	Special Funds	% Distribution: Pure	% Distribution: Applied	Mechanician
5502	$ { 10105.23 u; 21518.95 g }		$ 3000			100	0	Spec.
7435	30000	$2000	2000	$1000	$ 0	66	33	$2100
11083	22000	15	1800	0	0	100	0	450
3581	20000 g	200 g	?	1000?	0	95	5	?
234	20000 g	500 g	1700 g	100	195000			?
6812	20000 ?	?	1560	?	200 ?	100	0	1440
1599	20000 g	250 g	?	?	0	?	?	800
3921	15000	300	1500	?	9000	?	?	2000
8583	{ 5000 u; 8200 g }	?	1500	?	1000	100	?	1800
32769	10000	?	2500		5400			500
7423	10004.55	?	1400		2500	100	?	800
3873	10000	250	1100	—	0	90	10	0
772	10000 u	300 u	500	?	0	?	?	?
6582	8000	?	800	?	300	?	?	?
2656	{ 6000 u; 1000 g }	{ 100 u; 100 g }	1500	?	1000	100	?	?
11810	{ 2500 u; 5000 g }	{ 200 u; 500 g }	3000	{ 500 u; 600 g }	1000 g	?	?	400
2125	5000	200	2500	500	0	84	16	0
1773	5000 u	?	200 u	300 u	0	?	?	?
2448	5000 g	1000 g	1750 g	?	140000	100?		1200
1543	3869.11	50.25	300	—	1000	88	12	Spec.
560	3000	250	300	0	75	100	0	100
2424	2500	100	?	?	1000	50	50	
1500	2200	150	100	—		75	25	
457	2000	100	225	100	0	33	66	0
1100	1800 u	200 u	300	50	0	75	25	0
1777	1500	0	50	0	0	100	0	0
5000	1500 u	200 u	500	150	0	95	5	0
825	1500	60	150	250	250	66	33	0
1965	{ 900 u; 300 g }	{ 100 u; 100 g }	200	200	500	66	33	?
14461	800	250			0	100	0	
560	750 u	25 u		87				
465	100 u	10 u						
Totals	$292137.84	$7510.25	$30435	$4837	$358225	85.5%	14.5%	$11590
Graduate	101018.95	2650						
Undergrad.	45241.23	1135						

Legend: u = undergraduate; g = graduate; ? = uncertain. or found elsewhere; — = spread over both columns

In tabulating the results of the inquiry there were several difficulties which ought also to be mentioned. Many departments report that there is no standing budget, that the amount of money is conditioned by circumstances which vary from year to year, and that the amount allowed for any one year might be, for that reason, more or less than that allowed in previous years. Several institutions hope for better things in years to come and several others report that the budget is elastic and that they can obtain as much apparatus as is needed from time to time, both for undergraduate study and for graduate. Three colleges report that they have, properly speaking, no laboratory and no laboratory course, but one of these expects to obtain a well-equipped laboratory in the next two years. Another difficulty presented was the fact that in about six institutions no systematic survey had been kept for years so that the figures were, in many cases, more or less well grounded estimates. It is a pity that our academic institutions of learning do not follow the rigorous methods of reputable business concerns in keeping systematic inventories. Another ambiguity was the term 'apparatus.' The questionary asked for an evaluation "of psychological apparatus, models, charts, etc., in the laboratory, (exclusive of printed test material but inclusive of form boards, puzzle boxes, pictures, etc.)." It did not occur to the writer what status research animals would have in this connection but one director properly said that "animals were listed as apparatus." In general, too, it was difficult to define general laboratory equipment so one respondent commented: "Expenses and equipment are distributed among items which are separated in questionary and which vary with circumstances." An interesting feature was the amount of money available from student fees, especially in some institutions where the laboratory course is taken by almost 200 students. In some instances these fees go to make up the pay of the mechanician which brings us to another important consideration.

Chart 5 indicates, in general, the amount of money available for a mechanician but it does not show the variety of arrangements made for this important service. The laboratory budget may allow for any sort of arrangement from no mechanician at all to one whose salary approximates $2,000 for the academic year. One institution reports that such a mechanician produces apparatus valued at $5,000 for the year. Another reports that there is no mechanician available but one is greatly needed and that it is hoped to have one in another year. In a few institutions the person who functions in this capacity is not a technical mechanician but a general utility man, which situation reminds one of the practice in many European laboratories where the mechanician is at once a skilled expert in apparatus and a general secretary, doing clerical work for the department. Many laboratories have made arrangements with other departments which allow the services of a skilled individual, either as a flexible unit among all the scientific laboratories as in many of our smaller colleges, or as a prorated unit devoting anywhere from one-half to one-third of his weekly time to the demands of the psychological laboratory.

The following are a few comments of special significance:

"Year laboratory course 1 period (3 hr.) a week with registration ranging usually from 150-180."

"Experimental psychology here is at present chiefly incidental to the other psychology courses and clinic work. We are planning to make it an elective in science and to house the laboratory in quarters in a new science building."

"Practically all of our elementary students go through a 2-credit hour laboratory course."

"We have no apparatus exclusively for undergraduate work. All I can say about the annual budget is that we are allowed all the apparatus we need."

"I suppose Thorndike tests are given largely in the interest of applied science. Our department uses the records in many investigations that have no place, as yet, in applied psychology. Material used in demonstration in classes (paper tests) can not reasonably be put in either group."

Another interesting feature of the investigation was the encouraging amount of money which is available for special research under grants from the Rockefeller memorial funds and the Rockefeller Foundation or other private endowments, and through the offices of the National Research Council.

Of course, as in every classification so in the presentation of the net results of a questionary, a certain amount of damage is done to the individual items that appear. Gross distortion probably does not occur but almost every item has its own conditioning factors and its own complexion derived from local environments that are not fully described. It must also be noted that much of the material for pure psychology is extremely elaborate and expensive and that it is becoming more so with every refinement of method, as the writer well knows in connection with the direction that work in acoustics is taking, involving the refined circuits of the audion tube. One must not overlook the fact, however, that in applied psychology many of the better researches also involve expensive apparatus which is often borrowed from pure psychology. Then, too, the use of printed forms, if needed in great quantities, is not an inexpensive procedure when the more expensive calculators and the amount of clerical help are taken into account. It is also probably true that the demands of men equipped to apply psychology in the industrial world are becoming so great that their salaries are running far above the rates established for professorial work in pure psychology in our academic institutions. Our statistical experts in the colleges and universities of the country are high salaried men. Of course the problem is familiar to the heads of engineering schools, to the directors of chemical and physical laboratories, and to the chiefs of our geological departments.

In summary, it is worth noticing that psychology is apparently making definite progress in the establishment of its laboratories and it is safe to lay down the principle that the total inventory of apparatus in one of our larger universities ought to be, under average conditions, about $20,000 to $30,000, and that its annual budget should be about $1500. It has been stated by Cattell in the early report referred to, that the trustees of our academic institutions are not in the habit of reading the psychological journals, but let us hope that the directors of our laboratories do. While some instances of too great an outlay have occurred it is probably true, according to the observation of at least one person who has travelled widely in this country investigating laboratory conditions, that the directors of our laboratories do not have the courage to ask enough for the maintenance of their equipment.[12] Chemistry and physics are so well established that large sums are easily obtained for their

[12] It is encouraging to note that the writer of this paper has already received one inquiry concerning the equipment of a new psychological laboratory building, in connection with which it was said that the statistics presented in the reading of the paper, at the annual meeting of the American Psychological Association, would be helpful in obtaining funds for the equipment of the laboratory.

maintenance. Psychology has also come into its own. It is no longer referred to as the 'new psychology,' nor is the use of the experimental method questioned in connection with our introductory courses. At Iowa, for a decade or more elementary exercises have been required of the students in the introductory course in much the same way as laboratory work is offered in conjunction with elementary courses in chemistry, physics, geology, and biology. For that reason, those responsible for obtaining annual budgets should get into the habit not only of asking for more money proportionately, but should contrive ways and means of putting new pieces of apparatus actually into use either in the drill laboratory or in research work.

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THE ELEMENTARY COURSES IN PSYCHOLOGY

By MAX SCHOEN, Carnegie Institute of Technology

The writer hopes that this paper will not be taken as a criticism, but rather in the spirit in which it is intended and written, as a suggestion towards making the elementary course in psychology more significant. When possibilities of accomplishment are great but actual results are but meager, if not even mean, it is worth while to point out the contrasts and suggest the causes. The writer believes this condition to exist in the teaching of elementary psychology today, namely, that the course falls short of what it might accomplish, due to the general effect of one or more of the following defects:

(1) Beginning students of psychology are usually plunged into the sea of psychological data at random immediately on entering the course, and allowed to sink or swim in unknown waters as best they can. The result usually is that some students emerge from the experience fairly well drenched with isolated and, therefore, meaningless facts about the nervous system, perceptions, instincts, emotions, sensations, etc., some of which they retain more or less accurately, while the large majority of them harbor a pleasant or unpleasant memory of "having had psychology."

(2) The course is often treated as if it were but a preparation for more advanced work in psychology, with the result that certain topics and technical details are introduced that are of interest only to the specialist and confusing to the amateur. While it is true that the elementary course gives opportunity for recruiting disciples for advanced study, and no doubt succeeds in so doing, it is also true that by far the larger number do not go beyond the introductory course, and, therefore, detailed treatment of certain topics leads to mental indigestion, since there is not sufficient time to establish a relationship between the minute part and the whole from which it receives its significance. The result is, as a whole, that the student can not see the forest for the trees, and leaves many a topic in the course in a state of mental chaos. The two particular topics that suffer most from this fault are the nervous system and the sense organs. The *function* of the nervous system and of the sense organs, which is of paramount importance so far as psychology is concerned, is practically lost sight of in the maze of detailed structural analysis, which belongs strictly to anatomy and physiol-

ogy rather than to psychology. The suggestion is not that nerve and sense-organ structure should be omitted, but rather that only those items should receive attention, and only to such extent as to make the function clear and distinct. So long as psychology, as such, was short of subject matter, there was justification for physiological and anatomical detail in order to fill out a course, but with the recent development of a wealth of material of a strictly psychological nature, particularly on instinct and emotion, psychology can be emancipated from servile dependence on its neighbors.

(3) The course is often weakened by the adoption of a biased attitude which results in the presentation of a one-sided point of view of human nature. It is but natural, and even admirable, that the psychologist shall have personal leanings in favor of some particular school, but the introduction of the personal predilection into an elementary course is bound to defeat the purpose of the course, which certainly should be that of giving the student a well-rounded orientation in the field of psychology. After the smoke of the academic battle between structuralist, functionalist, and behaviorist has cleared, the common ground on which they stand comes to view, namely, that each seeks in its own way to clear up the most complex of all phenomena, human nature, and that each makes a most worthy contribution to the problem. Neither structuralist alone, nor functionalist alone, nor behaviorist alone, can possibly tell the whole story, for if one alone covered the whole field, the others could have never arisen. The very fact that more than one point of view exists is an indication that each one by itself tells but part of the story. Consequently, the delimiting of the course to a single school must lead to a one-sided, incomplete and partial outlook on human nature, human affairs, and human issues. Again, a good deal of conflict is just apparent and not actual, a difference in form and not in substance, in terminology rather than actual thought. There is enough of common substance, even in those issues in which the battle rages fiercest, upon which to build a well-rounded elementary course, without confusing the beginning student with the private battle of psychologists. The object of an elementary course can not rightly be to attract disciples to some personal inclination of the teacher or text-book, that is, to make behaviorists, or structuralists, or functionalists. Such a procedure is similar to practising dietary fads on a sickly child. The implication here is not that attention should not be called to these various points of view in psychology, but that any course is bound to be somewhat ineffective in proportion to the narrowness of the outlook adopted as the guiding principle. We can not afford to teach a school of

psychology, but only psychology, for whereas each school is right from its standpoint, prejudices, and inclinations, the standpoint of each by itself may be wrong.

(4) A fourth common weakness of the elementary course is its illogical sequence. The topics are seldom so systematically arranged as to follow one naturally from the other as a building-up process, and culminating in a conception of human nature as a unit. Most textbooks are of the nature of a series of essays on somewhat related topics, tied together by a common title. In a good many cases, a chapter might be entirely omitted, or all the chapters interchanged, without losing the sequence of the discussion. Once in a while, the phrase "as we have seen before" is introduced as a warning to the student that he is supposed to be progressing, but it is doubtful whether he either knows where he is going or why he is going, but only that he is on the way.

(5) Probably the worst defect in the elementary course, a defect that prevents the material acquired from actually functioning in the life of the student, is that, whereas the course is dogmatic in theory, it is evasive in application. The student might carry away a wealth of psychological fact, but is poverty-stricken in psychological value, since he makes no connection between life as it is lived and as it is pictured in his textbook. Even when a feeble attempt is made to apply a principle, it is found necessary to invent a fictitious John Doe and to place him in trite, unusual, even ridiculous situations. And yet the student is living his psychology while he is studying it. It seems that we are suffering from fear that our psychology will not be thoroughly scientific, and, therefore, keep it carefully away from life from which it is deduced originally. But of what value is a fact, as a fact *per se*? Of what value is learning unless followed by understanding? We do apply psychology to industry, salesmanship, politics, etc., then why not apply it to the business of living, to the functioning of the individual as an individual and as a member of society, that is, to individual and social affairs and problems? If the course is not to help the individual in the business of living, then what is it good for? Science for the sake of science is a beautiful ideal for the scientist who lives by his science, but for the layman it is but an empty slogan. Psychology should be the most vital course in the college curriculum, for, while other subjects might treat *about* life, psychology is the only study that is concerned *with* life itself. Our pride should not be so much that we are teaching psychology as a science, but rather that we are teaching psychology as life. If psychology is dehumanized, what relationship does what we teach bear to our definition that psychology

is the study of human nature? Human nature is what it does; then, if we separate the *what it is* from the *what it does*, what is left of human nature but abstract fiction?

(6) Finally, it is perhaps worth while raising the question as to whether there is any practical purpose served by introducing the elementary student to contentious material that is of private interest to the specialist, but which might only confuse the beginner, since he has no foundation upon which to evaluate problematical points. I refer to such items as theories of instinct, whether there are any instincts, etc. There has no doubt been a marked and wholesome change in our outlook on instinct, then why not present the general ideas and leave the refinements where they belong, to special treatises?

The Aim of the Elementary Course

Apparently, the success of the elementary course in psychology will depend largely upon a clearly formulated and serviceable aim. In the opinion of the writer, such an aim suggests itself as soon as one considers the status of the subject today.

In the first place, psychology is not only with us, but swamping us. Its popularity is so great as to arouse suspicions of superficiality, or even quackery. It has almost become a fashion, so that publishers claim that the word *psychology* on the title page of a book is sufficient guarantee for a substantial sale. Beside psychology, the vogue of electrons and relativity and heredity fade into thin air. Students enter the course with the haziest notions as to what they are to expect: character analyses, dreams, subconscious mysteries, phenomenal memories, overnight success, while the public in general consumes tons of printed matter consisting of rantings and ravings about what psychology will do for your business, your character, your children, your friends, your hidden powers, and your pocketbook. One of the main aims of the teaching of psychology should certainly be to counteract this quackery and exploitation, and to create a sane, wholesome attitude towards, and an intelligent reading audience for, psychology. Would not the accomplishment of such an end be more fruitful than the accumulation of a few facts that are forgotten—and gladly so—as soon as the pressure of the final examination is removed?

In the second place, it should be the object of the beginning course to inculcate in the student a tolerant, open-minded and broad attitude toward human affairs and human problems. There has probably never been a period in the history of man when such an outlook was needed more than today. Life has never been more complex, the very foundation of society never more seriously questioned, and the tension and conflict between individual and individual, between social, economic, industrial,

religious and racial groups, never more intense than at present. Since psychology deals basically with human nature, which is the source of all individual and social problems and institutions, there is certainly no other subject in the college curriculum that can present to the student as vividly and effectively the danger of bigotry, narrow-mindedness, prejudices, and pretenses in human relationships.

The problem is certainly at least worth while pondering as to whether the aims set forth above would not only make the psychological course more interesting and more vital, but also ultimately lead to a greater crop of industrial and social benefits than those garnered under the present general procedure.

The outline offered below is neither original nor revolutionary. It is simply the outgrowth of a serious attempt made by the writer and his colleagues to avoid the shortcomings, and to carry out the objectives, that have been discussed in the previous sections of this paper, and is here offered for whatever it may be worth as a suggestion.

Part I. Introductory Orientation: The Aim, Schools, Methods' and Branches of Psychology

The course opens with a discussion of what psychology is not. For this purpose the literature of the exploitation psychology is utilized in order to point out that this is an attempt to fleece the gullible and unwary by wild promises of benefits to be derived for little money and less effort by taking advantage of mysterious discoveries that will enable one to get something for nothing, or something out of nothing.

The next step is to establish an adequate definition of psychology that will admit of both mind and behavior. Such a definition is not reached arbitrarily, but is gradually arrived at by an examination of several problem cases in abnormal behavior. Briefly, the steps in formulating the definition are somewhat as follows: (1) The dominant trait of the human organism, as of all organisms, is that it conducts itself in certain ways under certain conditions. It is the aim of psychology, in the first place, to study the relationship that exists between these ways of acting and the conditions that call them forth, for the prediction and control of behavior. (2) But, behind all behavior there is a mind of some kind. Whereas, socially, behavior is the all important manifestation of human nature—since society must concern itself primarily with what the individual does, and not so much with what he thinks or feels—individual justice demands that an attempt be made to discover the type of mind that is behind a certain kind of behavior. (3) But mind is what mind does, that is, the sole evidence of mind is in overt behavior, and consequently we can reach the mind only through its external manifestations. (4) Therefore, psychology is an attempt to study the human mind in terms of behavior as scientifically as the complexity and variability of human nature will permit. (5) The value of the study of psychology is, then, that it leads to an understanding of human nature, and it is only where there is understanding that adequate regulation, control, and direction are made possible. An adequate understanding of human nature affords the key to an understanding of one's self and of others, and this understanding leads to self-control and to social peace and stability.

In the evaluation of the different schools of psychology, structuralism, functionalism, behaviorism, mechanism, and psychoanalysis, the purpose is

not so much to expound their differences and antagonism as to point out that, although they appear irreconcilable on the surface, they are fundamentally supplementary. Human nature is so complex, and presents so many angles of interest and attack, that any one point of view is bound to be partial and incomplete, and it is only when we synthesize all the schools that human nature, as it actually functions, is revealed. Each school is right from its standpoint, but its standpoint alone presents but a one-sided view of the whole picture.

The same procedure is followed in the discussion of the methods of psychology. Not introspection, observation or laboratory procedure is extolled or belittled by itself, but the limitations of each by itself, and the virtues of a combination of the three is indicated.

In the above scheme there is no marked departure from the usual procedure, excepting that the writer believes that this preliminary discussion is worthy of more time than is commonly devoted to it. In the elementary textbook there is at most one chapter given over to this introductory orientation. At best, the student can carry away but a hazy notion of what it is that psychology is after, that psychologists do not agree as to what they try to do or in how they shall do it. In a one semester, 18 weeks' course, fully six or eight recitations might profitably be thus occupied.

Part II. Motivating the Examination of Human Nature

Following the general orientation in the nature, the points of view, the methods and scope of psychology, an attempt is made to establish an objective for the analysis of human nature with which the main part of the course is concerned, and which constitutes the subject matter of the usual beginners' textbook. The aim is to inculcate in the student the point of view that human nature not only *is* something, but that it also, and primarily, *does* something, and that a clear notion of what it is can be obtained best by a clear understanding of what it does as a unit, an entity, and not in parts. In other words, that sensations, perceptions, instincts, emotions, and other elements of human nature, are the things we live with and live by, and not merely so much data to memorize and discard after the final examination.

This part of the course might be justly charged with being speculative and perhaps 'preachy.' The answer to such a charge might be that the prevailing procedure, which is factual and 'teachy' is certainly not altogether effective, and that a little of the speculative and 'preachy' might help to make the factual and 'teachy' more vital and significant.*

Part III. Analysis of Human Nature

The analysis of human nature opens with a discussion of the nervous system as the machinery of adjustment. The point of view adopted is that an adjustment to any situation is conditioned upon (1) becoming aware of a situation, and (2) reacting to the situation. Awareness and reaction are thus the two products of the mechanism which we call the nervous system, and are but two aspects of one and the same process, namely, from becoming aware of the situation to responding to the situation. Awareness is thus but a *delayed reaction*, and it is to this delayed reaction to which the general name of intelligence is given. The type and degree of the delayed reaction conditions the type and kind of response, and consequently intelligence is coextensive with behavior, and behavior with intelligence. Whenever the behavior is not of an automatic, trigger-action kind, intelligence of some type and to some degree is present.

The body of the course is then devoted to an examination of (*a*) intelligence, which begins with the vague general impression, namely, sensa-

*Pressure for space has made it necessary to omit at this point an illustrative syllabus of several lectures.

tion, and culminates in creative imagination; and (*b*) behavior, which begins with the mechanical reflex and culminates in voluntary, purposeful action.

The treatment is thus developmental and evolutionary, proceeding from the simple to the complex, from the crude to the refined, from the low to the high.

Under the general topic of intelligence are included in sequence, sensation, perception, imagery, memory, learning, and imagination.

Sensation is treated as the first, most elemental, contact of the mind with the objective world. It is, therefore, the grist that feeds the mill of the mind, and from which the finished product ultimately emanates. This first contact of mind with matter is of the nature of a general, vague impression, namely, that something is present to a sense organ.

Perception is this general impression becoming defined and specific. A sensation is thus useful only when, and in the degree to which, it becomes a perception.

Since all perception is in terms of what is already present in the mind, the next step is to examine the nature of this past experience that makes perception possible. This problem presents three aspects for consideration, namely: (1) the state of being of this past experience, or how we carry this past experience about with us, which leads to the topic of imagery; (2) how this experience is accumulated, which suggests the subject of memory and learning; and (3) the highest use that is made of this past experience, namely, in creative imagination.

A similar procedure is followed with behavior. Under this general term are treated, in sequence: reflex, instinct, emotion, habit, and voluntary action.

The reflex is the unit of behavior out of which all higher forms of behavior are developed.

'Instinct' is a general term applied to that state of complex behavior which antedates and precludes experience, training, or learning. 'Instinct' is, then, the condition of native behavior at a certain stage of evolution. Thus the 'instinct' of the ant is not that of the bird, and that of the bird is not that of man, neither in quality nor quantity. 'Instinct' begins with *instinctive behavior*, that is, a somewhat fixed form of behavior which is typical of organisms in the lowest stages of life, but which is somewhat amenable to modification by training and experience. The next higher step is *instinctive tendency to behavior*, which is more plastic than instinctive behavior, and hence more affected by training and experience. Finally, the highest form of 'instinct' is *a general inborn tendency to behavior*, reaching its climax in man, which is most subject to training and experience, and, therefore, man is the organism that can adapt itself to the most varied and complex forms of environment, and hence the most intelligent of living forms.

Habit is to instinct what perception is to sensation, namely, the general tendency becoming fixed as a specific reaction. The general tendency is thus valuable to the organism only when, and to the degree to which, it becomes a habit.

Voluntary action is to habit what imagination is to perception, namely, the highest use made of fixed behavior, or fixed behavior directed towards the accomplishment of a projected purposeful end.

Part IV. Final Synthesis of Human Nature

Here the organism as a whole, that is, the personality, is treated, as it functions normally and abnormally. The following are the main topics discussed in this section: the nature of personality, the measurement of personality, the nature and significance of individual, sex, and racial differences, the principal types of personality disturbances and abnormalities, and psychoanalysis as a theory and practice.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF ANTIOCH COLLEGE

IV. The Influence of the Group on Psychological Test Scores

By S. Burns Weston and Horace B. English

In Allport's experiments concerning the influence of the group on mental processes, tasks were assigned which do not have high correlation with intelligence.[1] In the present study, an attempt was made to measure this influence when the task assigned required considerable intelligence.

Two test forms of presumably equal difficulty were made by a random division of a group of 4 'intelligence' tests. Each form contained 16 items from Thurstone's Reasoning Test, 4 from Roback's Analysis Test and 4 from his Interpretation Test, and 12 from the Opposites Test devised by Brigham for the American Council on Education (1924 edition). Ten upperclassmen acted as *Ss*. The 5 individuals of Group I worked first in solitude on the tests of Form 1, then in a room with all 10 *Ss* on the tests of Form 2. The 5 individuals in Group II reversed this procedure: after

Table I

Group I			*Group II*			*Both Groups*	
Subjects	Score Alone	Score Together	Subjects	Score Alone	Score Together	Alone	Together
a	37	36	f	33	38		
b	32	39	g	35	40		
c	23	28	h	34	34		
d	23	31	i	25	34		
e	27	34	j	38	43		
Mean	28.4	33.6		33.0	37.8	30.7	35.7
σ distribution	3.07	3.82		4.34	3.49	5.42	4.22
σ Mean	1.37	1.70		1.94	1.51	1.72	1.34
σ difference of means		1.01			1.22		1.08
Difference		5.2			4.8		5.0

taking the tests of Form 2 in company with the *Ss* of Group I, they took the tests of Form 1 in solitude. When the *Ss* were working by themselves, the starting and stopping signals were transmitted by the experimenter from another room. Every effort was made to eliminate the effect of rivalry, and the *Ss* were not informed of the precise purpose of the experiment. In scoring, account was taken merely of the number of points correct.

Table I sets forth the results. The number of cases is small but the direction of results is quite unmistakable; 2 individuals did virtually equal work, the other 8 did much better when working in company with others. The difference between the means of the two solutions is so much greater than the standard deviation of the difference as to leave very little room for doubt that further samplings of similar subjects under similar conditions would yield the same result. Conservative practice insists that the difference should be at least 4 times the standard deviation of the difference, a criterion which is satisfied in this experiment.

[1] F. H. Allport, *Social Psychology*, 1924, 265 ff.

Approach to this problem was made in another way. Practically the entire student body of the college had been given an intelligence test prepared for the college by Dr. L. J. O'Rourke. In taking this test the students were together in groups of fifty to seventy-five. Later a random sample of nearly half the students had been given a group of three tests taken from the 1924 Intelligence Examination of the American Council on Education: Arithmetical Reasoning, Completion, Artificial Language. Raw decile ranks were assigned in both tests on the basis of the actual Antioch College distributions. Twenty-one students who had not taken the second of these examinations were selected on the basis of availability, so far as could be

TABLE II

Subjects	A. C. E. Decile Rank (alone)	O'Rourke Decile Rank (together)
A	10	9
B	1	10
C	1	1
D	8	6
E	6	8
F	7	7
G	6	5
H	6	4
I	8	5
J	2	8
K	1	4
L	5	9
M	6	10
N	10	5
O	4	2
P	4	2
Q	9	7
R	1	3
S	5	4
T	7	9
Mean	5.57	6.1
σ distribution	3.05	2.78
σ mean	.66	.61
σ difference in means	.322	
Difference	.52	

judged, a random group in anything affecting this experiment. These students took the examination in strict solitude. In the light of the previous experiment, we should expect to find their decile rank considerably lower in this test than in the O'Rourke test, since those with whom they are thus compared had had the advantage of working in the company of others. On the other hand, the student would undoubtedly consider this advantage a most dubious one. Table II gives the facts. As may be readily seen, the results are rather inconclusive. There is very slight difference in favor of the test done under group stimulation, but this difference is too small, in view of the variability of the means, to be reliable.

The second experiment is obviously not so clear-cut. The variables involved are not so well controlled. One does not see, however, what factor causes the difference. Further experiment is obviously seriously needed to determine the effect of group stimulation upon the processes involved in intelligence tests.

V. Notional *vs.* Rote Memory

By Mary Gretchen Jones and Horace B. English

Striking differences between the abilities of the same individual in notional and rote memory have been repeatedly demonstrated. We know, however, that the content of any bit of memory material, and still more its form, may tremendously affect the results. In the following experiment, we sought to ascertain the extent of the divergence when the material to be memorized and the form of procedure was kept as constant as possible. *S* was told that a short story would be read to him, after which he would be asked to "repeat as much of it as you can. It doesn't make any difference whether you remember the exact words or not, but you must listen carefully so that you can repeat as many of the ideas as you can remember." This constituted the test of notional memory. The story, a simple Chinese tale of but 91 words, was one that had been used by the senior author in giving tests of notional memory to children.[1] The selection was read slowly and with rather monotonous emphasis. *S* immediately repeated as much of the story as he could remember in his own words. By having the story written vertically down on the page, *E* could check in the column assigned to each *S* the several items recalled—a method which makes the checking decidedly easier and which, though rather obvious, does not seem to have been described for this test.

Upon completion of this first recall, *S* was told that he was now to learn the passage verbatim. Care had been taken to insure that this task was a surprise to him as we did not wish his recall in the notional memory test to be contaminated by an effort to recall the exact wording. The selection was read again as nearly as possible exactly as before. This was continued until the entire passage could be repeated verbatim. Fifty-six college students of both sexes were used as *S*s.

As arranged for scoring for notional memory, there were 31 items or 'ideas.' Even though the story is simple and short, there is a very considerable range, from 11 to 29, in the number of ideas recalled. The mean was 23.5 ideas with a S.D. of 4.17.

To master the selection verbatim required from 2 to 9 additional repetitions. The mean was 5.3 with a S.D. of 1.62.[2]

The scores for the two series showed a rather low positive correlation: $r = 0.328 \pm 0.08$ (Pearson coefficient). It is clear, therefore, that despite the several factors which would make for correlation, there is a distinct discrepancy between the ability to memorize the sense of the material and to memorize the exact words. Not only was the material identical and the procedure practically so; the first learning period was an identical factor in both tasks. If there is any common 'memory ability,' that too must have been operative in both. Yet divergence between the two tasks remains. Had it been practicable to correlate the notional memory score with the rate of improvement, the correlation would apparently have been lower still.[3]

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[1] H. B. English, The intelligence of school children correlated with social status, *Psychol. Mon.*, 23, 1917, 311 ff.

[2] Five additional *S*s, whose records are not included, failed to master the selection verbatim after 12, 16, 10, 9 and 9 trials. The notional memory scores of these same *S*s were respectively 25, 16, 25, 18, and 20.

[3] All these *S*s had taken two group tests of intelligence, the Thurstone No. IV and a test devised for Antioch College by Dr. L. J. O'Rourke. Both tests showed quite low correlations with the two memory tasks: Thurstone and Notional, $r = 0.12$; Thurstone and Rote, $r = 0.04$; O'Rourke and Notional, $r = 0.23$; O'Rourke and Rote, $r = 0.15$. These are probably too low to be of much significance; so far as they can be relied upon, they show again that there is a divergence between the two memory tasks. It was originally planned to treat the data by multiple correlation but the lowness of the coefficients makes this seem rather futile.

One must sharply raise the issue, therefore, as to the cause of the difference. Rote memory as we tested it is by no means solely the mechanical association of which Bergson speaks.[4] Yet if such mechanical association exists as a discriminable factor, there is clearly much more of it in the rote memory task than in the notional memory task which would seem to be dominated by the factor of 'meaning.' The results are of course far from conclusive for a variety of reasons. But they do seem to help put the burden of proof upon those who deny the Bergson-McDougall hypothesis of a difference between 'memory' and the mechanics of habit-formation by contiguity-association.[5]

VI. Reproduction and Eduction

By Marjorie Mack and Horace B. English

Among the several principles upon which Spearman proposes to establish the psychology of cognition is the "eduction of correlates."[1] Suppose that one is presented with a certain term or fundament and with the fact that it stands in a determinate relation with something else. One tends, says Spearman, to educe or discover the other term or fundament of the relation.

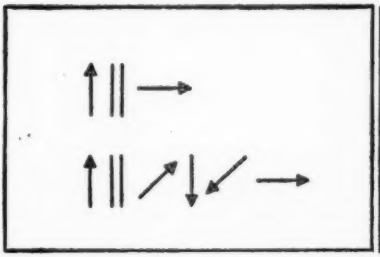

Fig. I

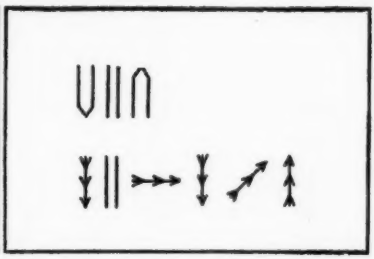

Fig. II

But the result of such eduction not uncommonly is exactly the same as if it had run off by the mechanism of reproductive association. If you ask me to give you a rhyme for *hill*, the word *rill* may come to mind quite as much from association as from any eductive, relational process. In other cases, however, it is clear enough that the 'educed' term has never previously stood in associational connection with the given fundament. It is Spearman's contention that such a process is fundamentally different from reproduction. If so, the two processes would have, almost certainly, different durations. Our problem was to test this derivative hypothesis.

In the first series reproduction of a previous associate alone was necessary, in the second the eduction of a correlate. Otherwise the conditions were as nearly alike as possible. In both series a fairly simple geometrical outline in two spatial orientations was shown in a Ranschburg mnemometer for two secs. There followed immediately the recognition-reaction

[4]Henri Bergson, *Matière et mémoire: Essais sur la relation du corps a l'esprit*, 1907, Ch. II.

[5]Cf. May Smith and Wm. McDougall, Some experiments in learning and retention, *Brit. J. Psychol.*, 10, 1920, 199-209.

[1]C. S. Spearman, *The Nature of "Intelligence" and the Principles of Cognition*, 1923, Chap. VII and *passim*.

presentation. This consisted of five figures in differing orientations. In Series I, the first of these, set off by a double line, repeated the first of the original pair while one of the four which followed the double line repeated the second of the original pair. (See Fig. I.) The task was the recognition and identification (by its position at first, second, third, or fourth) of the paired associate.

In series II, the task was essentially a kind of analogies test. The figures in the recognition-reaction presentation differed from those of the original pair. (See Fig. II.) But between the first of this presentation and the four following the same relation was presented as in the original pair. The task was the identification of the correlated figure—in the illustration, the fourth of course.

The *Ss*, psychologically naïve college students, were trained in a rather protracted practice series to react by releasing a key at the same instant they named the figure. With inexperienced *Ss* this is a far more natural method than the use of the lip key or even of a voice key. A few false reactions resulted but not enough to affect the results, and any time errors

S	Series I Reproducts Mean	m.v.	Series II Educts Mean	m.v.	*S*	Series I Reproducts Mean	m.v.	Series II Educts Mean	m.v.
A	4.6	4.1	3.4	1.4	M	3.1	1.7	6.0	2.2
B	1.8	0.3	5.0	0.3	N	2.4	0.4	3.4	1.6
C	2.9	0.6	3.3	0.7	O	2.4	0.9	4.5	1.2
D	1.9	0.4	5.4	2.8	P	1.8	0.2	3.6	1.1
E	3.7	1.7	5.6	3.7	Q	3.7	1.4	4.3	0.7
F	2.3	0.2	6.6	1.4	R	2.0	0.3	6.0	3.2
G	2.6	0.6	4.6	1.8	S	2.6	1.1	4.5	1.8
H	2.8	1.2	3.5	0.6	T	2.6	0.5	5.1	1.2
I	2.5	0.5	10.0	5.7	U	2.2	0.3	4.4	1.2
J	1.9	0.4	7.1	2.2	V	2.1	0.6	5.9	3.7
K	3.0	0.8	3.0	0.5	W	3.0	1.1	6.6	5.7
L	1.9	0.4	3.9	1.9					

Mean of 23 *Ss*: 2.55 m.v. ± 0.77 sec. for Reproducts;
4.72 m.v. ± 2.06 sec. for Educts.

were well within the range of the normal variation of the reaction. Each *S* had, after the practice series, six reproduction and six eduction reactions. The following table gives the mean reaction time in secs. and mean variation for each. We have included without penalty cases where the wrong figure was named. The reactions were timed to within a tenth of a second

It will be noted that the average reaction time in Series II is nearly twice that for Series I. Taking the *Ss* separately, 10 of the 23 need at least twice as much time for the recognition of the educted correlate as of the reproduced associate. Subject A has a longer reaction for reproducts only because one of her reactions ran to 15 sec., thus making her average a most unrepresentative figure. Eliminating that obviously atypical reaction, her average is 1.82 ± 0.3 sec. All others were considerably quicker in Series I. It is thus quite clear that for all *Ss* the second task was much harder.

"Of course it was." Yet one does not so easily say why. To apprehend the new figures of the reaction-presentation in Series II is a matter of a very small time, hardly significantly longer than the apprehension of the repeated figures of the corresponding presentation of Series I. That factor can hardly account for the difference. The eduction task was amply

illustrated and understood by all *Ss*.[2] Other accessory conditions seemed to us to be identical. Two seconds seemed entirely adequate for grasping the simple relation pictured in the pair presented prior to each reaction; the apprehension of the relation thus took place before the stimulus to the reaction was presented. *S*'s task in reaction judged superficially was merely one of recognition; in the one case of a previous associate, in the other case of a logical associate. But however much alike the two tasks thus seem to be, they are clearly different as psychological processes, as proved by their temporal course.[3]

We made no attempt with untrained *Ss* at an introspective analysis of the two processes. It may be that eduction is merely a very complex kind of reproduction and takes more time for that reason. Either introspective analysis or some other study of dynamic differences is necessary to decide this point. But even this preliminary study seems to make clear that there is an eductive process, hitherto unduly neglected, dynamically different from reproduction.

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[2]The relatively greater m.v. of the eduction series and some part of the lengthened reaction time may be due to an *initial* failure to comprehend the instructions. But inspection of the complete data show that this factor is not large and the final reactions are still longer for educts than for reproducts.

[3]Some degree of confirmation is afforded by the fact that there is practically zero correlation between the two tasks. Indeed such *complete* lack of agreement is rather puzzling on any supposition.

BOOK REVIEWS

Things and Ideals, Essays in Functional Philosophy. By M. C. Otto. New York, Henry Holt and Co., 1924, pp. viii, 320.

Twelve essays intended to make articulate a social philosophy "which recognizes equally the reality of things and ideals." How can aspiration be combined with loyalty to fact? Though of a semi-popular character, these essays show (in the opinion of the reviewer) an unusual amount of care and judgment regarding the crucial moral problems. They should come to the attention of those writers who are attempting a system of ethics based on Darwinism, Freudianism and statistical sociology. The book displays an unusual mastery of language—novel metaphor and analogy abound, but not at the expense of logical continuity. Otto rejects the notion that an absolute world view is possible. Philosophers must abandon their habit of rushing in where experts fear to tread. But philosophy, as a "dumb sense of what life honestly and deeply means," is feasible. The opportunity of philosophy, in view of the present chaos of moral ideas, is to achieve a new ideal.

Human nature is torn between two allegiances: one to fact, the other to the world of make-believe. An intimate sketch of the child's first acquaintance with this dualism is given. But men, due to the practical pressures of life, must leave the world of fancy, though some, like Lot's wife, are unable to keep from looking back. Others accept the commercial code of the age in which they live; and a few others, including the author, strive to reconcile the world of fact with the world of imagined ideals. In a sense, all are idealists—the only difference is in the ideal espoused. The moral idealist is not the only reformer—the 'possessive' idealist consumes as much energy as anyone in reforming the world to suit his own selfish ends. Otto deserves credit for pointing out the only dangerous philosophers—those who mutely practice meanwhile affecting to despise.

Three essays deal respectively with the idea of right for right's sake, with right as might, and with right by agreement. Kant's defense of right for right's sake is reviewed, then rejected as practically indistinguishable from right for any sake whatever. That is, since Kant fails to distinguish particular rights as the genuine embodiments of abstract right, anyone may accept him and then exploit an empty idealism by any practice, right or wrong. Nietzsche, Treitschke, and Bernhardi proceed by this method. The author accordingly rejects formal morality as having no guiding power in particular moral predicaments. He rejects also the idea that might makes right, after careful study of its arguments. The doctrine has two meanings: "It may mean that might is *essential to the triumph of right* or that right is *merely another name for the triumph of might.*" He criticises the second meaning of the doctrine. To consider moral belief as due to an antecedent fight is not to solve the problem of the identity of might and right—it only removes it to the past. On the other hand, to treat the winning cause as the 'strongest one' is a circular argument.

The author finds moral problems the outgrowth of the conflict of motives. It is the business of reason to create a new end in which the conflicting motives will find some manner of adjustment, though this may involve the sacrifice of some desires. The crucial point about this reasonable sublimation of desires is its creative character. In the following essay the author ventures a most original analysis of the moral points of view. To abbreviate, the author distinguishes three types of theory, but all of which concur in the belief that morality consists in conformity with some preexisting moral standard. The author develops a fourth view which "instead of making the moral life dependent on a previously existing moral order, makes the moral order dependent upon a previously existing moral life" (p. 113). This, if followed out, would lead to the radical view that

moral science moves in an unique realm, quite different from the realm of exact science. Whereas the facts of exact science are *discovered*, the realities of the moral sphere are *made*. Values, in short, have an unique existential status. They exist in a realm of becoming—facts *are*, values *ought to be*. Even wants only *are*. They do not, as such, generate a moral situation. It is only because they compete for realization that we can have a moral predicament.

The solution for the conflict of impulses consists in finding the 'richest' attainment of satisfied wants. Such a solution cannot be found once and for all. Also, moral conduct is an art—men should be allowed to cultivate individuality of wants.

Chapter 6 deals with the "Self." The difficulty of setting limits to the 'self' is pointed out. There is no specific date of its conscious origin, we define a man in terms of his whole ancestry only at the risk of neglecting the fact that a man is in a true sense his posterity. The author prefers to avoid this extreme and also the extreme of Hume who declares that the self cannot be found at all. The self, he thinks, takes its rise during childhood with the development of independent interests. Dewey's, "I own, therefore I am," is better psychology than Descartes' "I think, therefore I am." But among the many selves presented by our interests, we may recognize a true self in the paramount interests. Selves are not born. They are *made, created* through the contacts of experience. Granted that the self is an active interest; practically in moulding it, we are chiefly concerned with the conditions under which it develops.

Other chapters deal with topics of less significance for a review made from the psychological point of view. They are entitled, "Science and the Higher Life," "The Soul," "War and the God-Makers," and "Hunger for Cosmic Support."

Wells College C. O. Weber

Nozioni di Psicologia, con Preliminari Filosofici. By Giuseppe Tarozzi. Bologna, Nicola Zanichelli, Editore, 1925, pp. viii, 165.

Besides the philosophical preliminaries the treatise is divided into three parts with subdivisions as follows: (1) Spiritual Dynamics—a general outline of psychology: sensation; association; the unconscious, sleep and dreams; attention and psychic energy; (2) Thought—representation and perception; space and time; memory; imagination; idea, concept, word; judgment and reason; belief and reason; natural talent and genius; and (3) Sentiment and Practical Activity—action and appetition; sentiment and emotion; classification of the sentiments; the superior sentiments; desire and passion; instinct; will; the ego, personality, and character. The treatment of the topics is brief but rather comprehensive, and represents the theoretical and introspective point of view. Attention consists of concentrating associations on a given object and impeding the free course of association to other representative series; it is a foundation of every mental act, beginning with perception. Without attention the mental associations would not be able to order themselves in definite groups adapted to ends, and the mental work would be vague and unproductive. Hence the importance of educating attention. The force of which we are conscious in the efforts of mental concentration is cerebral energy and at the same time psychical and spiritual energy. Appetition is basic to psychic energy. It has pleasure and pain aspects; and pleasure and pain generate, but are at the same time generated by, tendency and aversion. Mind is appetition and will, force and energy, not only when it tends to action, but also when it seems to collect in itself the images and ideas in more perfect calmness. It does not gather but forms; it does not receive but creates. "Even when it is pure thought, and perhaps more then than ever, the mind is force which produces and works, operates, consumes, and reintegrates the energies of life" (56).

Human actions are divided into instinctive and willed acts. Reflex acts, which occur in instincts, lie in the field of physiology. Instinct is a manifestation of internal impulses, which either occur spontaneously or result as consequences of internal excitations which may or may not be representations and perceptions. Instinct as a fact is a series of complex acts making toward a certain end without knowledge of the end or choice of means; but it is not wholly unconscious. It is generally determined by the perception of an object, though this perception has not the function of a true conscious end. Consciousness accompanies but does not determine instinctive acts. Instinct is more invariable and definite in its expression than volition. Definitely determined acts are in the field of instinct. Will is essentially characterized by a determination of the mind toward a given end, whether positively by impulsion or negatively by inhibition of opposing acts. Thus the author takes the feeling of effort as evidence of a mysterious original force in will, rather than of conflict among opposing impulses. James is not mentioned though his views are often closely followed. Psychology as outlined in this book is a long way from becoming a natural science, being still beclouded by the rather free intermixture of arbitrary forces comparable to those ascribed to external nature by primitive man.

Peabody College for Teachers JOSEPH PETERSON

Prolegomena to an Idealist Theory of Knowledge. By NORMAN KEMP SMITH. London, Macmillan and Company, 1924, pp. xii, 240.

Comprising a reorganization of materials used in Mill's "Lectures in Philosophy" at the University of California, this book gives us a "formulation of an idealist theory of knowledge on realist lines." Much of the book is of an expository nature, devoted to a presentation and analysis of the views of many thinkers of prominence. The comments and criticisms of them are carefully considered. There are three chapters on "The Doctrine of Representative Perception;" one chapter on the "Nature and Functions of the Sensa;" a chapter on "The Categories;" a chapter on "Sense and Intuition;" together with an introductory and a concluding chapter.

The author shows, as he acknowledges, the influences of S. Alexander, Avenarius, Baron von Hügel, Bergson, and Stout. Out of these variant influences he constructs a theory of knowledge and a theory of reality of a clearly realistic sort. Ward's doctrine of a 'presentational continuum' is accepted with a rather radical modification. This consists in denying that the continuum is sensory. What is strictly sensory is not continuous at all. Time and space are the true continuities, they are objective, but not sensory. They are public, not private as are the sensa. The fact that (as Berkeley noted) time and space always appear with sense excitations does not prove that they are apprehended by the senses. The trait of space and time which makes them signally different from sensa is their never-failing implication of a larger space or a larger time in which they exist. Equally constant is their tendency to fall into smaller spatial or time units. The visual apprehension of space illustrates these points. The perception of size and distance is not conditioned entirely by the mere sensory impress. The same retinal impression may appear large or small, near or distant, depending on the attendant circumstances. Visual space is accordingly a matter of sense plus meaning.

As for the sensa, there are two views we may adopt regarding them: (1) either they are independently real, or (2) they are events which require certain physico-sensory conditions for their occurrence. He adopts the latter view. The author thus makes numerous concessions to subjectivism which most of the new realism of the present is striving to escape.

Wells College C. O. WEBER

Blinden-Psychologie. By KARL BUERKLEN. Leipzig, J. A. Barth, 1924, pp. 334.

This is a systematic survey of the mental processes of the blind, beginning with sensation and ending with personality. Some experimental results are given, as well as copious quotations from old and new writers, most of which are theoretical. Much space is given to an exposition of the normal psychology of each field as a background for differentiating the special case of the blind. The theoretical standpoint is that of structural psychology; for instance, there is an extended treatment of ideation. As a teacher of the blind, Buerklen is interested in the educational bearing of his subject; and in this connection, his account of the development of ways to teach the blind how to read and write is valuable.

Buerklen holds the view, supported by experimental data, that the differences of the blind from normal persons, aside from the fact of blindness, are not innate but acquired. This view is contrasted with the prescientific conception of *Sinnesvikariat*, according to which deficiency of sight is compensated by increased acuity of the other senses, either by divine favor or by drainage. Buerklen's position is obviously sound, except in so far as visual defects are innately connected with other conditions and that possibility seems remote.

Stressing the influence of environment, Buerklen attributes the superior acuity of the blind in hearing and touch, as well as their superiority in learning and retention, to practice. Their surprising powers of abstract thought are ascribed to their constant need of analysis as a compensation for the lack of sight. Environment, again, serves to explain the psychopathology of the blind; for they become conscious of inferiority, and are less well able to satisfy their impulses than normal persons. Buerklen describes the frequency and vividness of their non-visual dreams, both waking and sleeping, their great interest in religion, their interest and high powers in music and poetry, their tendencies to "sensitiveness" and "obstinacy," and their proneness to sexual perversion—conditions which in some measure are connected with repression.

Buerklen maintains that through literacy and a fit choice of occupations the blind can become useful members of society. He discourages the effort of the blind to become exactly like those who see, and points out that their defects require peculiar emphasis on their existing powers: the successful Braille alphabet, for instance, is based not on Latin characters but on configurations of points. On the other hand, he holds that knowledge gained through sight can and should be communicated to the blind; with its aid, they are capable of worthy achievement in music, poetry, religion, and philosophy. Because of the sensory deficiency, words are often the sole carriers of thought for the blind; and in any case, a special situation is involved in their acquisition of meanings. Caution must be used, therefore, in judging of the mental processes of the blind on the basis of verbal report.

Buerklen uses general psychology in interpreting the psychology of the blind: the converse relation of the special to the general, which is no less important, he does not explicitly recognize. Congenital blindness, especially is instructive to the psychologist, since it permits an increase of the knowledge of vision through a comparison of cases when it is present with cases when it is absent. The fact that the Braille alphabet differs from the Latin emphasizes the difference of the tactual perception of space from the visual. The effect of vision on the expression of the emotions is made clearer by a study of the expressions of the blind, which according to Buerklen are peculiar. Again, the powers which the blind acquire are within the reach of normal persons: if the blind have supernormal memories or logical powers or sensory limens, the normal can be trained to acquire them. Again, the psychopathology of the blind may be fruitfully com-

pared with that of persons who see; the results of repression are comparable whether the cause of repression be blindness or something else. The blind are no exception to the rule that the abnormal carries a message to the normal.

Harvard University J. C. KELSON

Die Vererbung geistiger Eigenschaften und der psychische Konstitution. By W. PETERS. Jena, Gustav Fischer, 1925, pp. viii, 400.

Psychologists who venture forth upon an exploration of mental heredity may lose themselves in a maze of statistics and speculation. To the credit of Peters be it said that he has left at least a guiding thread behind him. His is an attempt to apply to mental heredity those concepts and principles which have yielded such fruitful results in the field of genetics. He postulates a "larger biology" which includes not only the physical organism but the psychical as well and finds in this concept a point of departure for a science of mental heredity.

At the outset he defines psychology (p. 5) as the science of consciousness, of behavior and of individuality. He thus takes a perfectly safe and (shall we say?) sane position which leaves him free to develop his thesis without wasteful contention. Three progressive principles of genetics seem to him to be fundamental in the concept of biological heredity: the determination of similarities in related individuals, the continuity of the germ plasm, and the genotype. Throughout the book the geneticist will find a familiar terminology in such words as dominant, recessive, mutation, variation, germplasm, soma, *etc.* All these the author would carry over, with more or less modification, to the field of psychology.

Peters takes into full account the effect of environment (milieu) upon psychical traits and points out that those "milieu-factors" which operate upon the physical organism are not to be confused with those which operate upon the psychical organism. Among the former he lists such forces of nature as light, temperature, atmosphere, moisture, climate, subsistence; among the latter, the culture of the race, of the nation or of the family into which the individual is born.

Fully aware of the difficulties which beset the path of one who would apply the genetic method to an investigation of mental heredity, Peters points out that up to the present time psychology has made little progress beyond first principles, *i.e.*, the determination of similarities among related individuals. The chief methods for the collection of data which have been employed are those of direct observation by the investigator himself or by field workers, biography (the "Who's Who" method), the study of educational achievement, and the questionnaire. The unreliability of the last method is soundly scored and the suggestion is offered that for the investigation of mental traits the "situation question" is more reliable than the "direct question." For example, if it is desired to inquire whether a particular individual is avaricious or not, the questionnaire does not ask "Is he avaricious?" but it inquires as to the subject's reactions in a situation in which the given trait may manifest itself. The psychologist then forms his own judgment as to the manifestation of the trait.

In his discussion of heredity and environment Peters does not hesitate to call into question the conclusions of American investigators with reference to race differences. He says (p. 323): "Even if American investigations (based on intelligence tests) show a higher performance for the American whites than for the Indian and the Negro, the question still remains whether this inferiority of the colored races is due to an inferior native ability, as is usually assumed, or to a different 'milieu influence.' If Americans believe that a similar or even identical education assures an identical milieu for the whites, the Indians and the Negroes, they obviously overestimate the 'milieu influence' of the school and overlook unquestionably

different "milieu-factors' under which a child grows up in an Indian or a Negro family. Compared with such strongly fixed, traditional 'milieu conditions,' the influence of the school can be considered but little more than a varnish which is spread upon the surface of different materials and different structures. If the Negro were to construct an intelligence test conforming to the needs of the 'Negro psyche' and the Negro system of intelligence, the whites might appear on the average less gifted than the Negro."

The psychologist who knows his genetics will find Peter's book exceedingly interesting and suggestive; so also will the geneticist who knows his psychology. Not least among the good features of the book is an extensive bibliography which the author has attempted to bring up to date to the year of 1922. It is unfortunate that the volume has not been more substantially bound. My own copy is already in two hundred pieces, one for every leaf, a misfortune which I trust the publisher will note, since I should like to place the volume permanently upon my library shelves.

Harvard University C. E. LAUTERBACH

The Young Delinquent. By CYRIL BURT. New York, D. Appleton and Co., 1925, pp. xv, 619.

The problem of this book as stated by the author in his preface is "the psychology of juvenile crime." Its purpose is to give teachers, social workers and all interested in the moral welfare of young people "some notion how the criminal in the making may best be studied and handled." The material for the book is based upon actual case studies of juvenile delinquents made by the author, sufficient examples being given in the different chapters to afford concrete illustrations of the types of delinquency treated and the methods of investigation employed. While the writer has written in a popular rather than a technical style he has consistently maintained a scientific point of view and restraint in the handling of his subject. For the scientific reader copious footnotes supply a wealth of technical data and materials. Throughout the discussion methods of inquiry and treatment rather than results are emphasized.

In his first chapter the author describes the method of the psychologist in attacking the problem of the young delinquent which may be briefly stated as a scientific development of a complete case history. This is concerned with the offense, a careful examination of the child with respect to his mental, physical, and temperamental characteristics, to his environment, his past history, and to the possible means of reform including a follow-up study of results.

From the introductory statement of the problem and the general description of method used, the writer in succeeding chapters develops his problem from several angles, as: hereditary conditions; environmental conditions within the home and outside; physical conditions, developmental and pathological; intellectual conditions, subnormal and supernormal; temperamental conditions; general emotional instability; and sentiments and complexes. Following the discussion of each phase of the problem consideration is given to lines of treatment that might be prescribed.

The author stresses throughout the book the necessity of studying the child as an individual, of understanding his personality, and of making an analysis of his environment and its probable effect upon behavior. He points out the need of finding means of supplying and directing recreational and intellectual activities adapted to the child and indicates methods of diagnosis and treatment. Throughout the book the viewpoint is held that criminals are not born but rather that certain weaknesses or tendencies may be inherited which, given favorable conditions for growth, may develop into criminal traits. This view is strengthened by comparisons of the characteristics of delinquents with those of non-delinquent groups. Both

hereditary and environmental influences may have a part to play. Prominent factors in delinquency are poverty, overcrowded living conditions, defective family relationships, poor discipline, and lack of desirable recreational facilities.

This carefully written and well organized book should be of great value to all workers with children, delinquent or otherwise.

Cornell University E. N. FERRISS

Rechtspsychologie: Forschungen zur Individuel- und Massen-Psychologie des Rechts und zur modernen Rechtsfindung. By KARL HAFF. Handbuch der biologischen Arbeitsmethoden; Abt. VI, Methoden der experimentellen Psychologie, Teil CII, Heft 1, Angewandte Psychologie. Berlin, Urban & Schwarzenberg, 1924, pp. 118.

This is an inquiry into the psychological factors in law and in legal decisions. The main interest in the book to psychologists lies in the various opportunities indicated for applications of psychological methods, norms, and principles in the large and diversified fields of modern jurisprudence. The treatment is theoretical and critical. The author brings out emphatically the futility of basing the development of law and the rendering of legal decisions on 'pure sociology' and on subjective principles of right which, he points out, are but expressions of certain particular types of minds and experience. He maintains that the so-called public or common will is largely a fiction; that instead of being an expression of the wills of the people generally, it always bears the stamp of certain strong and more or less self-interested personalities. Even juries are so subject to this rule of the dominance of individuals that one needs not, to get a favorable decision, bring all the jurors to one's own side of the issue, but only those who give direction to the deliberations and conclusions of the jury. Modern legal practices demand that rural, industrial, social, professional, and other classes be investigated psychologically for the establishment of norms or representative types so that the practical demands of all forms of human living may be met more empirically and objectively. The importance of a practically exact evaluation of different mental abilities and maladjustments of individuals coming before the bar of justice is pointed out. The consideration and divisions of 'individual psychology,' and its separation from group psychology, treated largely as conceived by Le Bon and Sighele, suggest that the author is not wholly up to date in his conceptions of modern psychology. The book reflects a wholesome trend in legal practice toward better working relations with this basic science.

Peabody College for Teachers JOSEPH PETERSON

The Theories of Instinct: A Study in the History of Psychology. By E. C. WILM. New Haven, Conn., Yale University Press, 1925, pp. xiv, 188.

This book should have a real value for all psychologists and others who are interested in the development of the theory of instinct as a factor in animal behavior. Taking the theory of instinct hand in hand with the concept of evolution, the author starts with Thales and systematically traces them through all their vicissitudes up to Weismann and Bergson. It should be made plain here, however, that while, in *The Theories of Instinct,* the work is brought up to date, two-thirds of the book is concerned with pre-Lamarckian views, and that for a review of the great mass of quantitative investigation that has been in progress during the last half-century the reader must look to the sequel, *Instinct and Intelligence,* (now in preparation), where, presumably, this phase will be elaborated.

The book is written in an admirable literary style, to which, as a further contribution to ease of perusal, the publishers have added a readable size

of type, a dull finish paper and a wide margin. It is well foot-noted, and the material is well organized.

On the whole, the author takes a neutral position; but the reviewer would criticise his interpretation of the pre-Aristotelian Greeks, principally that of Anaximander, Empedocles and Democritus. The author takes too extreme a view in crediting these philosophers with such a modern viewpoint as he assigns to them. For example, he says of Democritus: "It will be abundantly seen, when we come to the discussion of modern theories of intelligence and instinct, from Descartes and Gassendi to Bethe and Loeb, that there is little in modern conceptions, apart from the elaboration of details impossible in Democritus' day, that was not expressed, in principle, in Greek atomism" (p. 15). "Apart from the elaboration of details" is granting too important a concession when, by this phrase, Democritus is exalted to the status of a modern evolutionist. As well try to make out of him a primordial Fechner from his use of the words "minimal sensible of hearing" (fragment 11, Diels). Furthermore, the reviewer doubts if the author is quite fair to Osborn, since the reader gets the impression that the latter held more of a brief for Empedocles than the reference (H. F. Osborn, From the Greeks to Darwin, 37-41) would indicate. The author says: "Empedocles is credited by Osborn with four modern evolutionary ideas." Osborn called them: "four sparks of truth," prefixing this with "a dim adumbration of the truth" and "by happy conjecture, gave his theory a semblance of modern evolution," which latter quotations the author omits.

A feature of his presentation is the author's emphasis on Reimarus, for which he is to be greatly credited. Reimarus has been comparatively unknown, and much neglected by writers on evolution and instinct. Yet he held remarkably well developed views for a worker in animal psychology at his time (1694-1768), and seems to have been quite a brilliant precursor to Driesch.

At the conclusion of the book is a critical comparison of "natural selection" and "the inheritance of acquired characters."

While the reader will occasionally find himself led to what seems the verge of total irrelevance, he will nevertheless be pleased to discover not a few masterly characterisations of men and concepts.

Harvard University A. RENDLE STONE

Complacency: The Foundation of Human Behavior. By ROBERT BRUCE RAUP. New York, The Macmillan Co., 1925, pp. xii, 201.

Complacency is the condition of dynamic equilibrium that constitutes the optimum state for the organism. "The first and chief business of living matter is to get into such relationships with other forms of matter as to maintain itself relatively constant while taking on their energy, utilizing it, and giving it off. . . . This satisfactory working relationship I have chosen to call complacency." It is the condition-from-which and the condition-to-which behavior is always in the process of moving. Annoyance is experienced when the equilibrium is disturbed by a stimulus; satisfiedness comes with the restoration of the optimum.

Evidence in support of the theory is cited from various fields. The irritability, conductivity, contractility, metabolism, and reproduction of living substance are all functions of complacency. A balance is maintained in the autonomic nervous system by the antithetical arrangement of the thoracico-lumbar and sacral divisions. Habit is the utilization of tried methods for the reduction of disturbances affecting complacency; a habit is fixed in accordance with the adequacy of its guardianship over equilibrium. Reflection works to facilitate the protection of complacency. In the realm of values, we find the individual prizes that which arouses disturbances for which he has reduction patterns ready. Man's good lies in the maximation

of complacency through the recognition of the interdependence of the disturbance and resumptive phases. The opportunity of education is to be found, therefore, in training each child to handle his own peculiar problem in the reduction of disturbances to his equilibrium, that is, to keep his poise among the variety of situations calling for adjustment.

The complacency theory is not a new one. Avenarius interpreted living processes in the terms of "vital balance;" Kempf, Jennings, Crile, Hering (visual theory), and Rignano (physiological invariability) have also advanced the concept of equilibrium—and Raup acknowledges their contributions. Koffka's theory of closure follows the same trend. Raup has weighted his discussion with a superfluity of quotation, though his citations are extremely well chosen. The choice of the term 'complacency' is an unfortunate one, since its ambiguity is deceptive.

Though the biological approach to the study of behavior is a valuable one, its limitations are of serious consequence, as we see in Raup's treatment of the problems which concern the psychologist. The neglect of the conscious side of complacency has robbed his discussion of much of its fruitfulness. In emphasizing the physiological aspect of the theory, the author tends to lose sight of the psychological side for which the hypothesis was advanced. The interest of the book tapers off at the end where the barrenness of the behavioristic interpretation discloses itself. While complacency is a worthy concept, Raup utilizes it so persistently that its inadequacy as the one unifying explanatory principle for psychology finally becomes apparent.

The book is interesting in the sidelight it throws upon the study of the mind-body relationship. Raup points out the intimate relation of psychology to the other sciences, and the 'complacency' theory indicates how signally the physical sciences can contribute to the progress of psychology.

Harvard University W. F. VAUGHAN

Zur Psychoanalyse. By EMIL RAIMANN. Berlin, Urban and Schwarzen berg, 1924, pp. 97.

This book originated with two lectures given by the author in Vienna, and is an account of the growth of psychoanalysis, with its extension into various aspects of everyday life, and a criticism of its methods and results. Now after thirty years of its activity in the Homeland, when psychoanalysis should be revealing something of its strength and weaknesses, we are told that its growth has stopped, except that the doctrine still unfolds itself in the luxurious phrases of its cult; that by far the greater number of all clinical directors as well as their students are in a greater or less degree, generally and in details, not adherents of psychoanalysis, but are in fact opponents of it. "Today there remains scarcely a single investigator worthy of the name who would go over to Freud's work." Freud and Breuer began in the nineties with the view that in hysteria the environmental greatly outweigh the hereditary factors, and they accordingly found in psychical trauma the causes of hysterical disorders. The unscientific nature of such an interpretation is emphasized. It takes a mere symptom, an accident in the individual's life, for the cause. These traumatic experiences do not cause hysteria, but the significance they take in the life of the individual reveals his predisposition to it, this being particularly evident in the sexual abnormalities dating back into early childhood. Recent war experiences make the Freudian position as to the sex basis of the various functional disorders untenable. A whole constellation of factors must be regarded as the cause of hysteria. If unpleasant experiences are so easily suppressed, pushed into the unconscious and dissociated from the normal conscious life, one may ask if the individual is not already a candidate for hysteria. Freud has mistaken symptoms for causes, and the degree of objective

validity of the memories which he traces back to youth that may exist, is but evidence of early weaknesses in the subjects concerned. It is inexcusable for Freud not to go more fully into the neural and general organic basis of these weaknesses. His methods have been those of setting up hypotheses or conceptions of the causes and then straightway treating these as established fact. The psychoanalyst has no objective standard as to which memories are significant, but he searches until he finds what he is after. Thus he moves in a circle.

Most of the usual criticisms, which we need not repeat here, are given by the author, who emphasizes the reasons for holding that the mental derangements and introversions found in paranoia and dementia praecox are due to actual physiological changes, some of which are now in a degree familiar to us.

While acknowledging indebtedness to psychoanalaysis for the great stimulus that it has given to the investigation of mental factors in functional disorders, the author holds that psychoanalysis has yielded no single result absolutely beyond the realm of doubt and uncertainty.

Peabody College for Teachers JOSEPH PETERSON

Talks on Psychotherapy. By WILLIAM BROWN. London, University of London Press, 1923, pp. 96.

This little book embodies three lectures given by Dr. Brown at the University of London in 1923. It is supplementary to his earlier books and is not a summary of them.

Lecture I treats of functional nerve diseases, dissociation, hypnotism, and suggestion. In this lecture, Brown discusses the nature and treatment of cases of shell-shock with which he came in contact during the World War and compares his view of the cases with the Nancy view and the Salpêtriére view.

Lecture II treats of psychoanalysis, abreaction and transference, and the libido theory. In this lecture the author discusses the merits and demerits of Freud's and Jung's theories of psychoanalysis and psychotherapy from the point of view of his own experience with shell-shock cases.

Lecture III treats of theories of melancholia, manic depressive psychoses, auto-suggestion and the will. Dr. Brown again refers to the theories of Freud, Jung, and Adler, whom he characterizes as the three outstanding authorities on psychotherapy of the present day. The lecture centers around the cause and nature of the mental conflict which produces the abnormal mental state.

An outstanding characteristics of all three lectures are the author's spirit of broad-minded fairness with which he treats the subject, and the pertinent facts with which he backs up the argument.

Harvard University C. A. SELZER

Die Menschheitsproblematik der Freudschen Psychoanalyse, Urbild und Maske. By EDGAR MICHAELIS. Leipzig, Johann Ambrosius Barth, 1925, pp. 123.

The author undertakes a personality study of Freud by presenting numerous excerpts from the latter's works. He points out apparent contradictions and the noteworthy refusal to admit considerations of the ego instinct into the orthodox doctrine. Michaelis believes that a study of Freud's personality will show a subjective basis for his 'onesidedness.' He is said to avoid the problems of the ego and the ideal not because he does not see them, but because he must suppress them due to his own complexes. "The most distant thing from every man is himself." Through

numerous quotations, he passes to a discussion of the universal "deep, unfulfilled longing for gratification of the ego ideal," which he believes to be the mainspring of human conduct. He closes with an appeal for the "expression of the ego's individuality, and true self, which in this way finds its fulfillment"—"which way the psychoanalytic doctrine has systematically barred."

University of Iowa GEORGE S. SPRAGUE

The Professional Treatment of Subject-Matter. By EDGAR DURRINGTON RANDOLPH. Baltimore, Warwick & York, 1924, pp. 202.

Cost of Training Teachers. By HOMER E. COOPER. Baltimore, Warwick & York, 1924, pp. 112.

In the first of these two volumes devoted to the training of teachers the author states his problem in this way: "Should teachers' courses in a specified subject-matter (of say, history, geography, arithmetic, or grammar) be given a special treatment that will differentiate them in thoroughgoing fashion from liberal arts courses in the same field? And if so, what should be the basis and methods of differentiation?" An affirmative answer is given to the first question, while the professional treatment of subject-matter is suggested as the best solution. This involves two essential ideas: (1) "on the side of scholarship such 'new view' of generally familiar material as will reveal its racial significance and its potentialities for public education, and such extensions of scholarship as will be most likely to insure flexible control of the problems of utilizing the study for social purposes in a particular teaching position; and (2) on the side of method it implies a conscious organization of instruction in subject-matter with reference to the professional responsibilities of the future teacher—which among other things involves the transmitting of a fruitful method of study and the purposeful affecting of attitudes and technique." Applications of this idea to geography, literature, and arithmetic are suggested.

The second volume is the result of a study of New York normal schools and presents data regarding existing teacher-training institutions, present cost of training, accepted standards of training, present status as to training, and requirements for meeting the standards.

Both volumes present valuable points of view and techniques to those interested in the training of teachers.

Cornell University JULIAN E. BUTTERWORTH

Skill in Work and Play. By T. H. PEAR. New York, E. P. Dutton and Co., 1925, pp. 107.

This book is intended for the elementary student, but the first two chapters are particularly of interest to the psychologist. Here the problem of kinaesthesis in human learning is isolated, and common psychological terms like knowledge, muscular skill, ability, and capacity are clearly distinguished. The thesis of the book is that our lack of accurate introspections of bodily experiences—a lack due to the fact that our vocabulary of learning is in terms of vision and hearing—is the principle cause of the difficulty in using kinaesthetic sensations as aids in learning. As a solution the author suggests the accumulation of accurate introspective reports and further investigations by means of 'motion study' along the lines set down by Gilbreth.

A critical consideration of the problem of acquiring skill occupies Chapter 2. Here Pear examines the theoretical conclusions of Freeman's *How Children Learn.* In a certain sense Pear's discussion is a critique of Freeman's work by way of amplification. Invariably Pear considers Freeman's conclusions to be too simple to explain adequately the complex problems involved. Pear objects to Freeman's emphasis upon the "series

of movements" to the exclusion of other factors, such as time-values, in the execution of these movements. He also undertakes an amplification of the psychology of learning by trial and error. He contends that it is improbable that there should be any mere stumbling by blind chance on the correct act when the "muscular activity involved has been properly discriminated, labeled, and characterized."

It seems to the reviewer that much of this difference ot opinion is a matter of point of view. Freeman treats the subject from the standpoint of original endowment in learning; Pear, from the standpoint of the efficacy of the teacher in imparting an acquisition of skill. Nevertheless, Pear's suggestions are timely as a critique of the notion that learning is primarily by trial and error and thus principally dependent upon chance.

Harvard University THEODORE KARWOSKI

The Passing of the Phantoms: A Study of Evolutionary Psychology and Morals. By C. J. PATTEN. New York, E. P. Dutton & Co., 1925, pp. 103.

This little book is written for the lay reader with a view to giving him "the master-key which unlocks the secrets regarding the evolution of human morality." It recounts instances of acts which are intended to illustrate the evolution of the "mental and moral faculties in lower animals." When the embryological and anatomical evidences of evolution have been given and the author passes beyond these to the evolution of mind, many errors and crudities appear. These are sure to give the lay reader a wrong notion of psychology and its methods. The point of view is that of the arm-chair comparative psychologists of pre-experimental days, and the author goes into disputed questions with the same certainty that characterizes the announcement of established facts. Thus we are told that man is a highly gregarious animal, and the origin of 'the social instinct' is traced. Most curious and naïve accounts of learning and of the unfoldment of sorrow, joy, and other emotional complexes are given. Evidences from the author's observation are marshalled to show that hawks have a faculty of attention and that persons have gained their "confidence and love"; that these birds love music and are strongly imaginative; that "imagination is highly developed in dogs;" that these animals, as well as horses, believe in spirits; that ants have moral sense; etc. Thus morality and religion have their pre-human roots of superstition. The author regards "the evolution of theology as an inherent instinct or inherited experience in the natural history of man, which will likely go on for an immensely long era yet to come." Surely this bit of psychologizing by a man of science unacquainted with psychology cannot be surpassed for errors even in the literature of the Fundamentalists, for whom, apparently, the book is written. There seems to be room for some popular writings on real psychology. It is a question whether literature of this kind is not more detrimental to scientific progress, and to psychology in particular, than the frankly anti-evolutionary books now circulating among the laity.

Peabody College for Teachers JOSEPH PETERSON

The Fundamental Principles of Learning and Study. By A. S. EDWARDS. Baltimore, Warwick and York, Inc., 1925. Revised Edition, pp. 255.

Those who are familiar with the first edition (1920) of this book will, no doubt, welcome certain changes that appear in the revised edition. The most important changes, perhaps, are a better organization of material and the addition of chapters relating to the higher type of education. The work has been brought into line with recent developments in that subject.

In this volume the author treats, in very brief outline, the general

field of the subject; but the theme of it all seems to be the *Habit Theory* of education. Habit, as used by the writer, refers "in the broadest kind of way to the permanent acquisition of the individual."

"The formation of so many thousands of habits as the aim of education is he says, "only a more clearly defined way of saying that the aim of education is the development of character . . . . for *character*, when it is analyzed, *is found to be the sum total of one's habits of thinking, feeling, and doing*" (42). Again "Education is ever the modifying and remaking, as well as the making, of habits" (56).

Several recent experimental investigations concerning the transfer of learning offer positive evidence in support of the author's conclusion that specific learning is economical and valuable; but 'general training' is little as regards quantity, and its influence is uncertain, for it may tend toward improvement or impairment.

Throughout the book, quotations from psychologists and educators are effectively employed in bringing out the central idea. The selected bibliography and the review questions are of significance to the student and the teacher.

The form of presentation is clear and pointed, so that the book might be used as a text in a short course, or it might very well serve somewhat as a guide to study and discussion in a long course.

Cornell University H. D. POWERS

Psychologie der Umwelt. By WILLY HELLPACH. Handbuch der biologischen Arbeitsmethoden, Abt. VI, Methoden der experimentellen Psychologie, Teil C, Heft 3. Berlin, Urban and Schwarzenberg, 1924, pp. 109-218.

An exposition, with desultory illustrations, of the logically possible methods of experimental psychology as applied to the study of the external factors conditioning human behavior. The three methods mentioned by the author are: (1) the method of simple observation; (2) the method of statistics (mass-observation); and (3) the method of experiment. He considers these methods with reference to the following three general classes of conditions: (A) the "natural Umwelt"—which includes weather, climate, ground or soil, and landscape, in a broad sense of this term; (B) the "mitmenschliche Umwelt"—the field of social psychology proper, interaction between persons; and (C) the "Kultur-Umwelt"—books, dogma, art, etc.

The methods discussed are too general to be of much value to the experimentalist. The treatise reads like the writing of a sociologist with addictions to logic rather than that of an experimental psychologist. He shows, also, a lack of discrimination in his choice of methods, as is evident from the five page discussion of "telepathy" as one of the methods of studying the interaction between individuals.

Peabody College for Teachers L. H. LANIER

Uber die Persönlichkeitstypen und die Methoden ihrer Bestimmung. By OTTO SELZ. Jena, G. Fischer, 1924, pp. 44.

The problem of types which has received much attention in Germany in the past decade, was surveyed by the author for the VIII (Leipzig) Congress of Experimental Psychology (1923), and is here published in a somewhat extended form. Particularly valuable are the fifteen supplementary pages containing a discussion of special points and a bibliography which, so far as the German work is concerned, is practically complete.

The author considers the methods of determining types under three headings, *viz.* historical science, psychology, and psychiatry. Recent progress in correlational work and theories of constitutional types are noted,

but it is clear that to Selz the most significant progress has been made along the lines of the prevailing concept of *Struktur*. Dilthey, Simmel, Jaspers, Troeltsch, and above all Spranger are given prominence for their development of an 'understanding' (as opposed to an 'analytic') method. Psychograms and studies of individual differences, these writers agree, neglect the integrating factors in personality. Selz makes the additional point that the syntheses in personality (typical interests and values) are in the last analysis the really crucial *determinants* in thought processes.

Harvard University G. W. ALLPORT

Textbook Selection. By R. H. FRAZEN and F. B. KNIGHT. Baltimore, Warwick & York, 1922, pp. 94.

On the basis of five criteria—interest, comprehension, permanent methods of study involved in the text, permanent value of the content, and mechanical construction—the authors develop a score card for rating textbooks in biology. In spite of certain artificial features in this particular score card, the use of such a method of evaluating textbooks should, by directing attention to significant factors, result in wiser selection. The latter part of the volume is devoted to reporting two studies: one on applying the criterion of interest to high school English literature texts; the other on applying the criterion of comprehension to geography texts for elementary schools. The entire volume is interesting as showing efforts to use scientific method in some of the less objective phases of education.

Cornell University JULIAN E. BUTTERWORTH

A Text-book of Psychology for Nurses. By MAUDE B. MUSE. Philadelphia, W. B. Saunders Co., 1925, pp. 351.

'Psychologies' are written now by anyone and for everyone. This book purports to present 'reaction psychology' in an abbreviated form so that it can be assimilated readily by nurses in training. Abbreviation scarcely can be recommended as an aid to clarity. There are many facts and much information in the book that probably is of value to the young nurse but the psychology is not what it might be, as the following quotations bear witness. "The patient's sense of hearing: No sound by which the sick are tortured is more exasperating than the sibilant buzz of whispering." "The *range* of attention is from the pathological distractability sometimes termed 'flight of ideas,' to a fixation of attention that is likewise pathological."

Hobart College FORREST L. DIMMICK

Ethics in Theory and Application. By HORATIO W. DRESSER. New York, Thomas Y. Crowell Co., 1925, pp. xvii, 509.

Distinguishing ethics as a normative science from descriptive sciences such as biology and psychology, Dresser assumes that the self or personality is real and that the moral order, including moral values, duty, obligation, and conscience, is a part of the ultimate structure of the universe. He concludes, among other things, that individual goodness means realization of our powers through organization of our many-sidedness, orderly development and expression through dominance of a purpose, that no matter whether the actions of the individual are conditioned by his heredity and environment, still the moral consciousness admits no evasion of individual accountability, and that sin is an irrational fact which can not be explained by heredity and environment. The contention that ethics is a normative science forestalls psychological criticism; but one suspects that this immunity is a curse rather than a blessing.

University of Chicago C. M. PERRY

Suggestion and Mental Analysis. By WILLIAM BROWN. London, University of London Press, 3rd. edition, 1923, pp. 176.

Psychotherapy has had two relatively exclusive lines of development, that of psychoanalysis, which includes the word-association methods and dream analysis, and that of suggestion, which takes various forms from autosuggestion to hypnoidal and hypnotic suggestion. These lines the author sees as converging, both as to theory and method. The discussion is largely elementary.

The book would have been improved by a revision of the sixth chapter and the omission of the section (chapters X-XII) which is devoted to a consideration of Bergson's philosophy.

Harvard University — E. G. WEVER

Elementary Psychology. By ARTHUR I. GATES. New York, The Macmillan Co., 1925, pp. xiv, 594.

The author offers this text as a general parallel to his earlier one which is designed for students of education. The book is one of a type of psychology texts which has been appearing in numbers recently and which assembles facts and theories from all sorts of psychological and near-psychological sources and attempts to present them without bias. This author has been successful in some measure, though the treatment of Instinct, Habit, and Learning betray his dominant interest in Education. The chapter on 'Methods and Subject Matter' is better than most of such discussions. The description of the organism is very good and is up to date and many of the new illustrations are excellent. The treatment of 'Sensations and Feelings' is however, inadequate. The last two-thirds of the book follow the educational and mental testing trend of the times. The material given there is an excellent digest of recent work in those fields and its results. The impression one gains of the text is that it is a very usable collection of valuable facts from psychology but that it is far from being a systematic presentation of psychology itself.

Hobart College — FORREST L. DIMMICK

The Humanizing of Knowledge. By JAMES HARVEY ROBINSON. Published for the Workers' Education Bureau of America. New York, George H. Doran Co., 1924, pp. 117.

For those concerned with the problem of the systematic democratization of science Professor Robinson's little book should prove interesting. He discusses the position of science and scholarship in modern society and suggests reasons and remedies for its opposition by many lay minds who fail to understand what it is all about.

To some persons this book will appear as an attempt to explain why Professor Robinson wrote his *Mind in the Making*; for the author urges a less austere attitude on the part of scholars to popular but careful treatment of their subjects, on the grounds that such treatments are of great educational value and indirectly aid research by interesting those who are in positions to offer material aid to science.

In the last chapter specific suggestions are given as to how and by whom popular treatments of learned subjects should be written, and as to how publishers can coöperate in this form of adult education.

Harvard University — HUDSON HOAGLAND

Were You Ever a Child? By FLOYD DELL. New York, George H. Doran Co., 1921, pp. 202.

The author points to many of the glaring failures in methods of teaching in public schools; and presents, in popular style, the new educational aims which are bringing revolutionary changes into those class-rooms which are not afraid to throw off tradition.

Harvard University M. B. PRATT

Leitfaden der physiologischen Psychologie in 16 Vorlesungen. By TH. ZIEHEN. Jena, Gustav Fischer, 1924, pp. 653.

This is the twelfth edition of a well-known work. From a modest volume of some two hundred pages in 1890 the book has grown to formidable proportions. The present edition contains 653 pages and 85 illustrations. Naturally Professor Ziehen has availed himself of recent investigations in the field of physiological psychology to bring his outlines up-to-date. Several former editions of the book have been translated into English. It is to be hoped that the twelfth edition will be accorded similar consideration.

Harvard University C. E. LAUTERBACH

Der Künstler. By EMIL UTITZ. Stuttgart, F. Enke, 1925, pp. 64.

Four lectures: (1) The Problem of a General Science of Art; (2) The Artist's Creation; (3) Art and Mental Disease; and (4) The Character of the Artist.

Introduction to Educational Psychology, A Manual for an Introductory Course. By HOWARD TAYLOR. Baltimore, Warwick and York, 1925, pp. 172.

This syllabus is designed for use with any standard textbook in educational psychology. Every chapter presents a quite complete list of secondary references, an outline, and a list of problems and questions. Every other page is left blank for student's notes.

The Psychology of Accidents and "Symptomatic Actions." By SAMUEL A. TANNENBAUM. New York, The Tenny Press, 1925, pp. 92.

An entertaining and effective criticism of Freud's theory of mishaps as announced chiefly in his *Psychopathology of Everyday Life.*

Intelligence in Expression. By LEONE VIVANTE. Translated by B. Bullock, with foreword by H. Wildon Carr. London, C. W. Daniel Co., 1925, pp. ix, 205.

This book is concerned with neither 'intelligence' nor 'expression' as the terms are ordinarily used in psychology.

NOTES

Mechanism, Meaning, and Teleology in Behavior: A Rejoinder

The reply to Dr. Weiss' reply[1] to my article[2] is that it is not a reply. The reasons for this are that when he considers specific points he is generally not talking about the same things I am, and when he discusses the larger questions involved it is on an assumption that shows clearly that he has not understood the point of view from which the article was written. I shall consider his paper from these two points of view.

I

Beginning with the latter of these topics, I want to repeat what I said in the article, namely, that I consider the "introduction of theories of reality into the work-shop of the scientists" a delusion and a snare. It is necessary to emphasise this because the difference between Weiss and myself, so far as our points of view are concerned, is exactly here. He maintains that behaviorism needs a metaphysical support if it is to be capable of scientific development; I hold, to use his own phraseology, that to bring philosophy 'downward' into science interferes disastrously with the primary tasks of describing, analysing, and generalising the concrete material with which, in the particular sciences, we are directly concerned. That Weiss seems not to be aware that our main difference lies here is a little surprising because it was given a prominent place in my article, and because it was on this very ground that my criticism of McDougall's advocacy of "purposive striving" as a *psychological* category was undertaken. The only difference between Weiss and McDougall, at this point, is that they appeal to different historical philosophies in order to make a 'go' of their psychologies, but neither of them escapes—Weiss less successfully than McDougall—the deadening influence of this initial blunder.

With this in mind, I am sure that anyone who reads Weiss 'reply' must be grateful to him for adding to the gayety of his fellow psychologists who, as a rule, do not have an unlimited opportunity for indulging their sense of humor. For, in the first place, Weiss is as serious as a Mediaeval Scholastic when he is confronted with his "materialistic monism," and is almost reverential in his gratitude for the light it sheds on the details of behavioristic doctrine; and, in the second place, there is something almost ecclesiastical, not to say pontifical, in the way in which he politely hurls anathemas at my head when he gratuitously assumes that, in principle, I am guilty of the same kinds of sin as himself in another cause. Since, however, as a psychologist I stand outside the wranglings of the philosophic schools, and am quite unable to understand how philosophy can be useful in developing into its details any empirical aspect of reality, I may be pardoned for wondering why Weiss thinks it is all right for the psychologist to be a 'scientific monist', and all wrong for him to be a 'dualist,' a 'vitalist,' or by whatever other name the philosophy which is not 'scientific monism' may be called. My wonder is not lessened when I observe that this obvious inconsistency determines the kind of reply he gives to certain sections of my paper.

I shall group together the passages in Weiss' reply which show (*a*) the influence of materialistic determinism in Weiss' theory of behavior, and (*b*) those in which he mistakenly supposes I am guilty of substituting philosophy for science in my criticism of behaviorism.

(*a*) To the former belong: (1) The reference to Child and Lillie. This is in answer to my statement that the present situation in biology leaves the

[1] Dr. Davies on "Mechanism, meaning and teleology in behavior," this Journal, 37, 1926, 450-459.

[2] Mechanism, meaning and teleology in behavior, *ibid.*, 2-24.

question of materialistic determinism an open one for the psychologist. No two names, and no more complete and representative list of authorities, *on either side the question*, can be made the basis of more than an *ex parte* statement. Weiss apparently thinks otherwise, and he does so, I think, because he has already accepted a philosophical position which seems to be supported by what the writers named say with respect to the material that falls within the limited fields of their respective investigations. It should be fairly clear that the truth of materialistic determinism, *as a theory of reality*, would not be settled with the acceptance by all the scientists of the deterministic position. Perhaps only then could we begin to discuss the question with a clear understanding of the problems involved! As matters are, I do not think the psychologist should take a gambler's chance which is the only option open to him at the present time. (2)Statistics, Weiss argues, necessitate mechanical causation, even in social life. This raises the question of the relation of mathematics to the particular sciences, and ultimately to philosophy. Historically and in practice mathematics have provided a useful method for investigating nature, but itself does not and cannot tell us a single thing about the constitution of any fact that belongs to the realm of nature. It directs our investigation of nature, but does not give us our doctrine of nature. Moreover, the deductive philosophies, based on the mathematical ideal, from the time of Descartes to that of Hegel, should be for every one a sufficient warning. Statistics, which are a part of the mathematical equipment of science, are subject to the same general limitations, and their value lies in their ability to give rules of averages—not scientific laws; or general tendencies—not specific principles of behavior. They are not, therefore, useless, and I wish merely to point out that it is impossible to deduce from a statistical law the modes of behavior of any concrete individual, and that this should be possible if Weiss' contention is correct. (3) Weiss is under the impression that "mechanical monism has made physics, chemistry, biology what they are," and for that reason, he says, it is worth trying in psychology. That is, in principle, much like saying that because quinine is useful in malaria, a common cold, and loss of appetite, it is worth trying in a case of broken bones. His contention would be valid only if the material of psychology were the same in kind as that of physics, chemistry, biology; but, as I pointed out, it is exactly that question that the behaviorist has begged by his acceptance of a materialistic metaphysics. Moreover, Weiss' statement of the relation of the particular sciences to monism is incorrect. The sciences advance according to the degree of their ability to develop specialised techniques adapted to the specific subject matter of each of the sciences, and are able to describe, analyse, and generalise the aspects of reality that come within their respective fields, and not at all in the degree of the correctness of the metaphysical view of the universe that particular scientists happen to hold. Sir Oliver Lodge, for example, believes in the existence of a spirit world in communication with this planet, but whether true or not, this philosophical belief has no relation to his work as a scientist. Why, therefore, the psychologist should burden himself with scientific monism which is as irrelevant to his work as spiritualism, as Weiss wishes him to do, is not at all clear. (4) Weiss practically admits my fundamental contention in this section when he says that in defining behavior as a form of motion, he does intend to commit the psychologist to the philosophical view of materialistic determinism. I disagree with him entirely, first that the scientist is a philosophical propagandist, and secondly that science halts until the ultimate mysteries of the universe have met with a true—monistic, dualistic, or pluralistic, spiritualistic, materialistic, or immaterialistic—explanation. I contend that, as psychologists, we are nothing but scientists.

(*b*) I turn now to those passages in which Weiss avoids a direct question which has a scientific signification by the assumption that, like himself, I am talking from the standpoint of an accepted metaphysics. Here

belong: (1) His denial that the scientist is concerned with the reality or actuality of his data. Is the question of actuality scientifically irrelevant? Does it make no difference whether, to use his own illustration, the responses Weiss makes to a stimulus are actually the responses that he does make, and whether these responses preserve their original character when in turn they become stimuli for other responses? Does it make no difference to the chemist whether the elements that are in his test tube have a fixed character, and whether the reaction between the elements is of fixed configuration? If such elementary logical requirements as these are neglected, or if they are irrelevant, how would it be possible, as Weiss contends, for either chemist or behaviorist to check "the degree of reliability (that belongs to their respective observations) by the degree of correlation which exists between the various individuals making the same observations"? The fact is that the question of reality, the assumption of a world of objective existence, is an important and fundamental necessity if science is to exist; it is only in pure mathematics, that the question of reality is irrelevant. For pure mathematics, as Bertrand Russell says, "consists entirely of assertions to the effect that if such and such a proposition is true of anything, then such and such another proposition is true of that thing. It is essential not to discuss whether the first proposition is really true, and not to mention what the anything is of which it is supposed to be true... Thus mathematics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true." When, therefore, Weiss argues that all questions of reality are irrelevant for science, he throws himself open to the retort that he is contending that behaviorism, like mathematics, "never knows what it is talking about, nor whether what it is saying is true." (2) The problems of the secondary qualities. My contention, put in the form of a syllogism, was that Weiss' advocacy of materialism ruled out, without injury, this problem, and that therefore he could not have an open mind as to the kinds of thing that might possibly exist in the empirically discoverable world of reality. Yet Weiss thinks I was arguing "to introduce the syllogism into the study of human behavior"! Would it have been simpler to say that the syllogism was my "verbal response" to his "materialistic determinism" as stimulus? (3) It is again the philosopher in Weiss that leads him to suppose that I urge upon psychologists the acceptance of metaphysical dualism, albeit in an attenuated form. He also hints, in the latter part of his paper, at my advocacy of the "spectre of vitalism." Either, it would seem, any stick is good enough to beat a barking dog, or there is unearthed here a philosophical monstrosity worthy of a place in a museum of curiosities, and in that case Weiss should have made more of his 'find.' (4) The implications of the term 'meaning' we are told, "are obviously non-scientific," by which I understand Weiss to mean 'philosophical.' As if I had not warned the reader against such an interpretation of the term! I had even said that what was required was "a study of the *mechanism* by which what is new in the conditions of life is met by behavior that goes beyond the limits of heredity. The question underlying this study is whether behavior ever exhibits this characteristic." How can my belief, or want of belief, in 'dualism' be an answer to this question? It is, of course, as irrelevant as 'materialism.' (4) The problem of perception is dealt with by Weiss in the same way. We are told that it is "loaded" so as to include a "non-mechanistic factor." Of course, for me perception is just as objective a problem as the transit of a star, it concerns the observed characteristics of behavior; it is one of several classes of behavior of which reflex action, instinct, emotional dispositions, etc., are others. All I am interested in is a description of the observed characteristics of the concrete instances that are agreed upon as perceptual. It seems so difficult for the behaviorist to understand the necessity for this! For, as Weiss admits, my illustration of a 'city street' is "too complicated for a *behavioristic* (italics mine) analysis," and he proposes to substitute

"learning to dance" which, he says, is not "essentially different." In this he concedes my point, because in the former, the conditions of behavior are never twice alike, and, therefore, the behavior must in every case be different, and, in the latter, the conditions and the behavior are fairly standardised. The question, we see, has nothing to do with "loaded" definitions, but with the "loads" that concrete facts themselves carry.

II

In this section I shall deal with certain misunderstandings with regard to (*a*) the logical foundations of science, and (*b*) specific statements, whether right or wrong, of my article.

(*a*) Under defects of logical method I include: (1) The descriptive poverty of behaviorism. As Weiss understands it, behaviorism is largely a theory of how particular reactions take place, not a description of what the reactions are. I do not mean that the latter question is entirely neglected, but that the importance the former is given—based chiefly on the sucess that has attended the study of the simpler forms of reaction that have a close analogy to physical reactions—has operated in a prejudicial way when more complex reactions are in question. These are assimilated to the type of the simple reactions and treated as such. As a question of methodology, there is no objection to this procedure provided that complexes are analysed into their simpler component reactions, and, as in the analagous case of the parallelogram of forces, these are resolved *seriatim*. But if this were done, it would be found that, as in the case of the illustration, the final resolution gave no clue to the particular constituent forces, that these could not be recovered from the former, as would have to be the case if materialistic determinism were to prove of any value in the theory of behavior. (2) Behaviorism is weak in preliminary analysis, and neglects or entirely overlooks the conditions under which behavior takes place. This false abstraction from the conditions which make behavior possible gives the impression that behaviorism is manipulating a set of very generalised concepts according to a set of formal rules. One illustration of this neglect of conditions is seen in Weiss' discussion where he says that "the statement, 'behavior is a form of motion,' is both a biosocial stimulus and a biosocial response," meaning by 'statement' in both instances the light pattern of the words on the printed page. Now, of course, the light pattern is the same objective fact in the one case as in the other; and it is impossible to say whether it is a stimulus or a response until the whole concrete situation in which the 'statement' functions has been completely described. If this were done, it would be more obvious than it now seems to be that it is exactly the difference in the contexts of identical patterns that gives to these patterns whatever meaning they may have: 'physically identical' does not necessarily mean 'psychologically the same.' It is, in fact, a careful regard for conditions that makes science possible, for as, Whitehead has recently remarked, "the key to the process of induction . . . is to be found in the right understanding of the immediate occasion of knowledge in all its full concreteness. . . We find ourselves amid insoluble difficulties when we substitute for this concrete occasion a mere abstract in which we only consider material objects in a flux of configurations in space and time. It is quite obvious that such objects can tell us only that they are where they are."[3] (3) Behaviorism is not critical of its own foundations. This comes out nicely where Weiss remarks that demands of this sort "are merely stimuli produced by movements of the speech mechanisms which may or may not be investigated with respect to their own sensorimotor antecedents," and where he brushes the question aside by saying that "philosophers have no sense organs which the physicists do not have," and that, therefore, "they cannot see a 'reality' which the physicists are unable to

[3]A. N. Whitehead, *Science and the modern world*, 1925, 64.

see." It is a mathematician who has recently warned us that "if science is not to degenerate into a melody of *ad hoc* hypotheses, it must become philosophical and enter upon a thorough criticism of its own foundations."[4] I had already insisted upon this in regard to behaviorism when I said that if it is to undergo a "systematic development" of doctrine, it had to submit to or undertake the task of "immanent criticism," that is, to turn the light of reflective criticism on to its own foundations. Weiss does not see the necessity for this, and he uncritically accepts the doctrine of materialistic determinism into conformity with which, he says, we *must* shape our psychological doctrine if we wish to be described by the sacrosanct term 'scientific.' Determinism is Weiss' *latente Einstellung*, and it not only narrows the scope of his science, but renders him incapable of recognising the negative instances, for instance 'memory' to take Russell's example, that may interfere with the smoothness of the tale.

(6) A selected number of misunderstandings of specific statements and questions is the following: (1) The question of consciousness. It ought to be clear from what I wrote that I do not employ the term 'consciousness' to imply a substantialist theory of mind. Further, no one who has read the literature of the subject since James' *Does Consciousness Exist*? should suppose that the denial of this view necessarily relegates the term to limbo. But this is what the behaviorists seem to do: they say, either you must mean by consciousness an existential something, or you must mean nothing. It was this position that I challenged in my article. Weiss' reply has not relieved me of the impression that the behaviorist is neurologically blocked on this subject in the 16th. century Continental view of the problem, and that he forgets that the English empirical school at that time were saying something of a very different sort. No one, of course, wants to confine the discussion of this subject to its historical antecedents, but too many seem not to be aware that history may sometimes save us from talking nonsense. The behaviorist's attitude toward consciousness is difficult to understand: he ignores history and refuses to adopt the empirical position, and, on the basis of a heightened emotional complex, falls into a sort of hysterical blindness whenever the word is mentioned. This is extremely unfortunate, because it deprives us of the criticism that is necessary if the facts that the word denotes are to be brought to a clear focus. I indulged the hope that help would be forthcoming. My remarks were irenical, and I went as far as I thought the behaviorist would be willing to go in advocating "a strictly empirical attitude" in the matter, and I suggested a mode of procedure that might be followed without prejudice to the conclusions. Weiss, however will have none of it; he rejects my "mental order," which was offered as a more neutral term than consciousness, even as a starting point; and he quietly but promptly dumps the terms and the problems they suggest overboard. It is clear that this attitude is based on a misunderstanding of what the discussion involves, and the misunderstanding is emphasised when he supposes that I was or am interested in "the content of the term consciousness." It is very remarkable that he should suppose this, because, in the paragraph from which he quotes, I had said definitely that we should empirically "collect specific instances" and "describe their characteristics." From this and from the whole tenor of the article, it should have been obvious that I was not concerned with the meaning or connotation of consciousness but with its denotation. To what characteristics of behavior, if any, does the term point? That was and is the question to which, if the behaviorist pays any, even a controversial, attention, we have a right to something more than a categorical denial or a wholly partisan answer. (2) The term 'apprehension.' When Weiss says that scientifically I can only mean by this term, or its correlate consciousness, a verbal stimulus and response, I can merely say that he is entirely mistaken. It should also

[4] Whitehead, *op. cit.*, 24.

be said that I did not use the substantive but the verbal form, and that this is more in conformity with my general position. I am, however, rather frankly suspicious of the "speech reactions" of the behaviorists; they appear to me largely as empty formulae; they seem to function as a sort of Hegelian 'absolute' that obscures and swallows up all the concrete differences in our human experience in the interests of an abstract monism that mistakes itself for science. But there is another point, in this connection, that needs clearing up. I used the equivalent of the phrase "non-physical form of apprehension," and because he failed to interpret it in the light of the context in which it occurs Weiss fails to understand what the term denotes. Taken by itself, it may mean either that the process by which something is apprehended is non-physical, or that the apprehended object is non-physically present. Which of these two senses was intended should have been obvious, since the sentence which precedes the one from which Weiss quotes reads: "Or can he [the behaviorist] describe and react to it [a material object] in *its* physical absence?" I was instancing the case of 'image.' What I wanted to know was whether for behaviorism the object was always present in the sense in which it is said to be present, for example, in perception; and, if not, what account was to be given of this type of behavior, and, further, what status the object would, in such a case, be said to have. Instead of answering these questions, Weiss merely says: "It is only when imagery is forced into such categories as 'apprehension' that we get the apparently absurd condition of reacting to a non-physical condition," and that he reacts to Mr. X "in the same way" when he is absent as when he is present in the flesh; in other words, he misses the whole point in question. (3) Weiss' long statement of the meaning of the term 'environment' is not, in principle, different from mine; it is, in brief, only the application, intentional on my part, of the doctrine of the universe of discourse which is expounded in elementary books of logic.[5] I think it sound. But, first, he neglects the concrete question I raised, and secondly attributes to me a position I do not hold. The neglected question is whether we are to define 'environment' as consisting of everything outside the reacting organism, or whether it is some part of this. For either a given organism's reactions are the mechanical resultant of the sum-total of existing forces in the universe, or they are the resultant of some smaller number of these forces. In the latter case, the number of forces that become effective must depend in part upon the reacting organism and its active needs. I hold, then, that what is true of the particular organs of the body, namely, that they are functionally analytic with respect to the complex stimuli that play upon them, so that, for example, the eye picks out ether waves and the ear air waves, is true of the total organism, and that it, in like manner, picks out the particular aspects of its concrete situations which, when reacted to, satisfy the needs by which it is moved to act. From this it will be seen why I cannot hold the position Weiss attributes to me. He says that I wish to limit the term 'meaning' to a particular locus within the series of interactions which constitute the universe. I had said that I did not know where the limits, if any, are to be placed, and that our primary business was carefully to observe and accurately to report the facts as we find them in order to ascertain if the fact denoted by the term meaning was anywhere to be found in nature. I suggested that it was a characteristic of certain of our relations as behaving individuals to our environment, and, in a note, that I should probably place this characteristic lower in the scale of behavior than would Weiss. The danger, therefore, that Weiss points out does not really exist; rather, it lies in the opposite direction, for, so far from limiting it to a given locus, I should be more inclined to regard it, directly or indirectly, as a universal factor of anything that by careful definition could be called behavior.

[5] Cf. for example my *Textbook of logic*, 1915, 50 ff.

(4) Weiss accuses me of "arbitrarily placing all social action into a teleological category." An examination of the passage which he quotes from my article will show clearly that I was not concerned with the question implied in his criticism. I definitely confined my discussion to the limits that Weiss himself gave to his biosocial category. And besides there would be no point in attempting to prove a universal when my purpose could be met equally well by proving a particular. What the passage in question aimed to show was that Weiss' removal of biosocial phenomena from the category of the biophysical necessitated the use of some other principle of explanation than the mechanical for these phenomena, and I asked what this principle could be if it were not the teleological. It is exceedingly gratifying that there is no difference between Weiss and myself as to the answer to be given to this question. I am, for example, in hearty agreement with him in the summary paragraph where he says that social life aims at experimentally establishing progressively an "optimum ratio" between what he calls "labor" or group interest, and "leisure" or the individual interest. Indeed, I had pointed out the same contrast a number of years ago under the terms "personal" and "individual," and argued that social and moral progress depended upon the establishment of a certain balance between these forces.[6] There is more than external similarity between our positions, for Weiss admits, what is implied in both, that the fact that he denotes by his "optimum ratio," and which I denote by "balance," is, to quote him, a "*final* tendency" (italics Weiss'), and, of course, final tendency is exactly what is meant by teleology, even if Weiss thinks it helps matters along to hide the real facts of the case behind the reservation that "the scientist is justified in calling it mechanical." (5) The criterion of teleology, Weiss says, is not clearly enough stated. Does he mean by this that what is stated leaves him in doubt as to the kind of thing I mean by 'teleology,' or does he mean that I have not developed a final definition and justification for teleology? If he means the latter, I can only say that it did not accord with the nature and purpose of the paper; if he means the former, it is obvious that what I said was explicit enough as a stimulus since Weiss has responded with a larger number of actions to this than to any other single topic in the paper.

In conclusion, I wish to say that it would gratify me exceedingly if the reader were to take what I have written as a measure of my appreciation of Weiss' work, and if he were to interpret the sharpness of the contrasts I have pointed out between Weiss' position and my own as being due, in part, to the necessity of focusing attention on the points at issue, and, in part, to the importance of defining the problems common to behaviorism and psychology, and of doing this in such a way that all ambiguity is removed as to the facts that the terms we use are intended to denote. When we have dispersed the mists that obscure our problems and data, it will be time enough to inquire if behaviorism is better able to give an explanation of them than psychology. No doubt, we all have our opinions about that. I have mine. But that is irrelevant.

Colorado Springs ARTHUR E. DAVIES

A FINAL STATEMENT

It seems to me that instead of approaching agreement the discussion of "Mechanism, meaning, and teleology in behavior" is leading to reiteration and greater divergence. While a continuation of it would doubtlessly prove of mutual benefit to Dr. Davies and myself, I do not believe it would prove equally interesting to the readers of this JOURNAL.

Ohio State University ALBERT P. WEISS

[6] *The moral life*, 1909, 120-153.

The Visual Perception of Movement

Dimmick's reply (this Journal, 37, 1926, 461-463) to our criticism (*ibid.*, 63-115) of his study of visual movement (*ibid.*, 31, 1920, 317-332) deals (*1*) with matters of technique and instruction, and (*2*) with questions of terminology and interpretation.

Upon (*1*) we offer the following comments.

(a) The slight glow surrounding each of our stimulus-objects exercised no influence upon apparent visual movement for the perceived motion was exactly opposed in direction to any irradiation or shrinkage of light.

(b) The use of figures of our sort gave greater control than Dimmick's through the reduction of the visual field to two small lines upon a lightless ground. Under such conditions there was no grey flash, which is—as we discovered—an occasional phenomenon appearing under a particular arrangement of stimulus but not essentially related to the perception of movement.

(c) Our instructions were concise and exact. We called for "particular and precise statements as to the order and nature of the appearance and disappearance of the phenomenal object or objects" and demanded that *O* confine his reports to "qualitative, temporal and spatial properties and relations." These instructions are certainly no more ambiguous than Dimmick's "describe in psychological terms," or "characterize the perception as fully as you can."

(d) Since our *O*s were given intensive training of 80 observations each before any report was accepted and since *five* of them (as against Dimmick's *one*) had completed their doctoral training, lack of stability of report can scarcely be invoked to account for the differences in outcome in the two studies.

(e) While we grant that our experimental conditions were different from those of Dimmick, the most casual survey of our original study will show that we did not base our criticism of him upon any *comparison* of the two experiments. We first stated clearly that under our conditions we found the grey flash wholly irrelevant to the perception of movement. We then proceeded to indicate certain obvious discrepancies in his study by way of which he apparently arrived at his questionable conclusion. As we showed, his results admit of a wholly different interpretation.

Now we advance to (*2*), the interpretation of results.

(a) We did not mean at any time that kinaesthetic movement was visual movement. We suggested that the phenomenal successive objects might be integrated in various ways so as to give the perception of apparent visual movement. Where *O* consistently reported pronounced visual motion without any visual filling our problem was to show the way in which such movement occurs. It was in seeking an explanation for such cases that we said "in the absence of all visual items other than the perception of successive positions, clear movement was apprehended through kinaesthesis. These experiential qualities served to integrate the whole perceptual incorporation; that is to say, to join the otherwise unrelated visual constituents. They furnished an experiential background for the purely visual items to functionate in perception." Dimmick, on the other hand, does not attempt to explain his negative cases. He simply ignores them. Where he contends that the values "are parallel" we showed at 30σ there were 103 more cases of optimal movement than of corresponding 'process-configuration;' while at 150σ there were 135 more cases of 'process-configuration' than movement.

(b) Referring to the *quantitative* treatment of our results we explicitly stated that "the most obvious fact in Table I is the prevalence of cases of full-movement; *i.e.* movement from the position of one line to that of the other." We added, furthermore, that "it must not be assumed that full-movement always meant that a line was apprehended to move, *as a line*,

across the field; although this is the usual meaning of the term in previous studies. The *Os* instead reported at times that the experience meant that one line moved from one position to another; but that the actual movement *of a line* across the field was not seen. It was just *bare* movement over the field. It was an approximation to Wertheimer's pure *phi*." We sought here merely to say that we so considered full-movement as to include pure *phi*, where no object is present in the field of movement. Dimmick, as well as Wertheimer, allots a *separate* discussion to pure *phi*. We indicated that movement may be optimal without an object moving across the field *as an object*. The large number of such reports was included under the caption of full-movement. Now, what of Dimmick? *He did exactly the same thing as we*. He considered pure *phi* under optimal movement, for he says (italics ours), "finally, a number of cases of Wertheimer's pure *phi* were reported by our *Os*. *Frequently the line was not seen during movement; there was simply 'something moving' or just 'movement.' Sometimes neither of the lines was clearly perceived; just a bit of movement downward. The cases in which both lines were clearly seen with only movement between them are entered in Table I as optimal movement and are paralleled by total process-configuration.*" For statistical purposes, then, Dimmick included under optimal movement the same sort of experience as did Wertheimer and ourselves. He did this strangely enough when he replies that he "defined 'optimal movement' as the 'movement *of a line* all the way from one position to the other.'" This is indeed curious; to place for quantitative treatment under the head of optimal movement all the reports in which there was just movement and no line moving! Since this is the same type of movement which in our case we said was a clear approximation to W's pure *phi*, no difference can here exist. We can state specifically, then, that for statistical purposes we treated full-movement in exactly the same fashion as did Dimmick. He can scarcely hope to protect the integrity of his grey flash through any appeal to such imaginary differences.

(c) Concerning our criticism of the alleged correlation between grey flash and movement, Dimmick protests that his reports are "entirely straight-forward if they are read with a proper regard for our definitions." Consider only two of the various illustrations which we offered: (1) "An oblique line appeared, then a horizontal and the oblique moved down to the horizontal;" and (2) "An oblique line moved down to the horizontal but not smoothly; there was a flash of good movement at the beginning and end." The first is supposed to be *unimembral* and the second *bimembral*. The most casual reader will immediately see that we might, with equal justice, call both *optimal*, for a line moved all the way! We can scarcely admit these and similar cases as straight-forward and unambiguous.

(d) Again, it was not clear, how Dimmick proposed to account for differences in rate of movement as increasing and decreasing under a given exposition unless the *process* reports indicated a precise phenomenal basis for the reported difference in rate of *movement*. An inspection of the actual reports does not—as we hold—show in a single case any ground for this temporal difference. We say flatly that out of the 103 reports cited by Dimmick not one includes a description of the temporal aspect of the 'process' experience. Not once in the 103 reports do such words as "brief," "momentary," "instanteous," "short," or "rapid" occur in connection with the description of the grey. In fact, no descriptive temporal terms are given; yet the temporal aspect is as important for Dimmick as the qualitative. We ask again for the evidence to account, on the side of 'process,' for the difference in rate of movement.

(e) Finally, we must consider the most serious of all the statements in Dimmick's rejoinder. We remarked that "since Dimmick found the shift in process-configuration to be directly dependent upon the temporal interval and since any shift in the process pattern or the temporal interval

meant, as he thought, a corresponding change in the type of perceived movement, it is strange that he does not cite *figures* or other testimony to the alleged change of optimal over into unimembral and bimembral movement as he passes temporally in either direction from the optimal interval. *At no place does he show the number of cases of unimembral, bimembral, and intramembral movement at various temporal intervals, i.e. the relation between these movements and the time variable.*" To this he replies that "it would be strange had we not done so." We insist that it is strange that he did not do so, and stranger that he should now imply that he has. He says that "Tables I and III of our report (321 f.) furnish the desired data." But let the curious read try to verify from these tables Dimmick's statement that the integration is "directly determined by the temporal interval between the exposures of the two stimulus members" by attempting to determine the cases of unimembral, bimembral, and intramembral configurations and movements. Remembering that Dimmick used *five* intervals, let him try to work out the number of such cases say at 30 or 150σ. What will he find? From Table I he will learn that each O reported so many cases of each kind. But what significance has this for the *particular temporal interval?* Table III shows *only* the cases of optimal movement. From neither table is it possible to obtain data which *deny* our statement that Dimmick does not "cite figures or other testimony to the alleged change of optimal over into unimembral and bimembral movements as he passes temporally in either direction from the optimal interval. At no place does he show the number of cases of unimembral, bimembral, and intramembral movements at the various temporal intervals, *i.e. the relation between these movements and the time variable.*" Dimmick admits that it would be strange if the data were not there. The tables to which he refers do not give them. The reader may make his own inference.

It is impossible to review the other adverse criticism in our experimental study which Dimmick attempts to meet. Those who are interested will find exposed in our original article various internal weaknesses in his data and conclusions. The reader will readily comprehend that it was but a final error on the part of Dimmick to regard our investigation as confirming "the results of his earlier study."

University of Illinois GLENN D. HIGGINSON

ON OVERLOOKING FAMILIAR OBJECTS

Professor Titchener concludes from his own experience in overlooking familiar objects[1] that Fuchs's theory is not applicable to most situations. Fuchs's notion was that the familiar object was obscured because its own average color scheme was absorbed or 'levelled out' through assimilation to the surrounding object-environment. Titchener has not only observed no color change in the overlooked object but gives positive evidence against the theory. His explanation is of two sorts: (1) the visual image does not fit; (2) the motor response is not started by the proper stimulus.

In a number of observations on the subject since this note appeared I have found that the explanation of my experiences tends to conform to a special case of an inadequate visual image. An object that is not in the looked-for place can not, of course, elicit an automatic motor response of the sort that Titchener describes. The problem resolves itself, then, into the following stages: (a) the letter opener, blotter, or eraser is not in its usual place on the desk or work-table, and the habitual movement that is initiated becomes inhibited; (b) the hunt begins for the object that fits the visual image. I agree with Titchener in the explanation of the first stage which is largely then a matter of motor 'set' and may lead to some sort of verbal expression couched perhaps in the vernacular, but in my experience the second stage has almost always been reduced to a further analysis of the unsuitable visual image.

[1] This JOURNAL, 35, 1925, 304-5.

Now the chief feature about the mental image that makes it inadequate seems to be its size—it is too small. The situation is similar to the familiar experience of doing problems in simplest arithmetic or in spelling most familiar words on the blackboard before the class. The writing seems strange and hard to follow because of its abnormal bigness. When I stand off at a distance the situation is quite satisfactory and the sum is easily made or the words spelt. It is obvious that the sensory experience does not tally with the average size of imagery usually attached to the perception. A contrary experience occurs when passing through the mountains on a train, we see, perhaps, a deer on a distant mountainside, or when crossing the ocean or a large lake we pass another boat at a distance on the water. If, in either case, we try to photograph the object we are usually disappointed in the smallness of its size and are quite convinced that we saw the object many times larger. There is here, as it appears, an interpretative or a perceptual attitude which makes the image much larger than its physical reality. These cases may not be identical in all their elements but illustrate a sort of attentive working over of the material.

At various times, I have had altogether five experiences in which the overlooked object on the shelf or on the table was squarely in front of me, and in roving over the visual field I invariably looked through it. When it finally dawned upon me the humor of the situation lay in the fact that the object was unusually large as compared with the picture of it that I had in my mind. Of course, the fact that the observer looks through this object or all around it and thus disregards it is a well-known fact of attention illustrated by the attentive disregard of objects seen by the other eye when both eyes are open in microscopic work or in sighting a gun at a target. At least one psychologist has told me that when he has no use for an individual it is not true that he purposely disregards him on the street. It is much more accurate to say that he does not *see* him. Of course, we have further evidence of this sort of thing in pathological cases where individuals will not note objects at hand but will nevertheless successfully avoid them in making their passage through the room.

This problem could be very neatly worked out on an experimental plan and the hypothesis verified to see whether the size of the object is the chief error in the hunt. In talking the matter over with a colleague still another explanation was offered which does not, however, fit my own case. It was that the total configuration of the objects on the desk was attended to rather than the analysis of the pattern into any one particular object. In my case I have a distinct and very vivid image, perhaps not of the type of 'recurrent image' described recently by Dallenbach,[2] but nevertheless very clear and of a definite size, although the size is wrong.

University of Iowa CHRISTIAN A. RUCKMICK

WHEN AND HOW TO REPLY TO A QUESTIONARY

Many psychologists are being embarrassed by the increasing frequency with which they receive printed or mimeographed questionaries on scientific, historical, or administrative matters. The questionaries often come from unknown persons whose ability to make adequate use of accurate replies is not known. They are often of a form that renders accurate reply impossible. Frequently the reply would require considerable research of a minor character on the part of the person making the answer, and most psychologists feel that they would rather determine for themselves the distribution of their own time in investigation. Sometines the questionary even asks for information that is as readily available through library sources to the author of the questionary as it is to the recipient of the questionary.

[2] This JOURNAL, 35, 1924, 155; cf. also E. B. Titchener, *A Beginner's Psychology* 1915, 75.

It is plain that many of these questionaries partake of the character of a 'nuisance.' For this reason some of us in four of the larger psychological laboratories have drawn up, under the title of this note, a set of rules which constitute in a way a code of action in this matter. Within a few weeks after the printing of these rules psychologists in six other large laboratories have asked for copies for use in replying to questionaries. Since this attitude toward the promiscuous questionary seems to be so general, it seems desirable to print the 'code' in the JOURNAL, so that the principles upon which some of us have agreed may be available to all, so that a reply to a questionary may sometimes be made by reference to the JOURNAL, and so that publicity may tend to correct the error at its source. The author of this note will, moreover, have reprints of it, which he will be glad to supply *gratis* to psychologists who desire copies as long as the supply lasts.[1]

1. If the questionary interests you or can be answered accurately in a few minutes, reply as a simple matter of courtesy, unless you see some postive reason why you should not reply.

2. Do not, in general, reply to a questionary issued by a person whom you know neither personally nor by scientific reputation.

3. If the unknown author of a questionary is introduced by someone whom you know, the case may be different; but you should make sure that the author's sponsor is actually guaranteeing the value and the precision of compilation.

4. Do not reply at all if you cannot reply accurately. Do not encourage slipshod work. If the questions are ambiguous, omit them. If the questionary calls for 'yes' or 'no' and the answer in not truthfully categorical, do not reply categorically, but give the complete answer or none at all. If there is insufficient space left for an accurate answer, omit the answer or use an extra sheet. If the wording of a question is ambiguous, omit it. If, however, there are many equivocal questions or many instances of insufficient space, it is a sign that the questionary has been made without sufficient knowledge of the situation under investigation and is, therefore, unlikely to yield reliable results; do not then reply to it at all.

5. Do not reply when the questionary asks for information that is available to the questioner through library sources, unless you know the answer without looking it up.

6. In general a request for a reply should be by personal letter to you, and not by mimeographed or printed form.

7. Do not think that the receipt of a stamped envelope for return of the questionary, or thanks expressed in advance, places an obligation upon you for reply.

Harvard University EDWIN G. BORING

URBAN'S TABLES EVEN AGAIN!

Dr. S. M. Newhall points out an error in Davies' correction of Rich's checking table (this JOURNAL, 34, 1923, 312). The value, when $p=0.27$ and $x=+2$, should be 4.9665 instead of 2.9665. The error is quite obvious, and in fairness to Davies it must be said that it is not of his making. The error is typographical, made by the printers during the 'make-ready' after the 'oked' page proofs had been returned to them.

K. M. D.

ERRATUM

In the recent article by P. R. Farnsworth, "Ending preferences in two musical situations," 'mi' should have been printed throughout for 'me.' Cf. pp. 237-240 *supra*.

[1] See also resolutions passed on the matter, this JOURNAL, 37, 1926, 468; or *Science*, 63, 1926, 474; or *Psychol. Bull.*, 23, 1926, 292.

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